Forum Series on the Role of Institutions in Promoting Economic Growth

When is further reform growth-enhancing?

A diagnostic tool and an application to trade liberalization

CLIFFORD ZINNES AND J. ROBERT SUBRICK

Forum 5
Final Presentation of Applications

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14 February, 2003 (Revision: 6 May 2003)

Executive Summary

How much reform can a country take and still have a positive impact on economic growth?

As veteran reformers always suspected and unlike the dictum of Economics 101, "more is *not* necessarily better". For as many as there are reform success stories, we find an equal number in which the impact of reform has been at best neutral and in many cases discernibly negative. A concrete example is privatization in the former centrally planned economies. According to a recent USAID study (Zinnes *et al.* 1999), the macroeconomic impact of privatization was not uniformly positive. It turned out that over the decade of the 1990s, only where the underlying supporting institutions–those of corporate governance, regulation, and hard budget constraints–were of adequate quality was privatization growth-enhancing. Similar though perhaps more qualitative findings have been observed for fiscal reform and decentralization.

Such results should really not be so surprising. The purpose of reform is typically to increase a country's economic efficiency by shifting resources and labor across sectors. This entails having institutions with two characteristics. First, they should be flexible enough to facilitate the capture of the benefits of reform–e.g., by expediting rather placing obstacles in the way of resource reallocation. Second, they should be strong enough to adjudicate redistributional impacts and to minimize the short-term negative externalities from the greater demands on infrastructure, congestion, and natural asset degradation.

A model to gauge impacts of further reform

We build on these insights and propose a method for USAID to gauge the likely impact at the macroeconomic level of further reform in a particular area or sector, i.e., whether more reform would likely be growth-enhancing. The method is based on an evaluation of the combined past reform experience of countries for the sector under consideration. We do this by creating an indicator of the quality of the policy regime in a sector, computed as the (standardized variance of the) unexplained performance of the sector under analysis once non-discretionary characteristics (e.g., culture, geography, climate, quality of rule of law) are purged. We then use the indicator to benchmark countries over time, both compared to all countries as well as to the relevant country cluster. Then, by analyzing country scores relative to macroeconomic performance, we are able to compute, based on country characteristics, country-specific thresholds that indicate when further reform is likely to improve, worsen, or have uncertain effect on macroeconomic performance.

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Method provides sector- and country-specific "rules of thumb"

These country benchmarks and thresholds, which one can think of as "rules of thumb", may serve several useful functions for USAID. First, they provide a starting point for discussion on whether a country should implement further reform in the particular area when other, more detailed, studies are unavailable. (In fact, even when such studies are available, they tend to be country-specific and use different methodologies, making comparisons difficult.) Our rules of thumb should therefore be helpful for mission directors either who are just arriving in country or who want an independent, comparative assessment for a new sector. Second, the rules of thumb should be a helpful *orienting* device for USAID/Washington staff that participate in parameter-setting meetings on diverse countries and sectors and don't have time for extensive preparation. Here the rules of thumb provide an independent–and cost-effective–reference point with which to evaluate the economic assessment or proposals in the country assistance strategy document. When a reform is proposed for a country well below its institutional threshold then the country should exhibit positive idiosyncrasies that compensate for apparent inadequacies relative to other countries in its group. However, the operative term here is "orientating". While the rules of thumb describe the expected performance based on countries with a similar set of initial conditions, they are *not* an alternative to fielding a sector preparation team. Finally, USAID may use the benchmarks as flags to identify countries as good candidates for further reform in a sector or to identify countries whose rule-of-law institutions themselves need strengthening before making future progress on particular sectoral reforms.

Application to trade liberalization: "one size (policy) does not fit all" – institutions matter

As an example, we describe and illustrate a diagnostic toolkit which applies our methodology to the current debate on whether it is wise for a particular country to undergo further trade liberalization. As is often the case in such controversies, we find that both sides are "right"—depending on the initial conditions. Starting with our full sample of 80 countries, we first show at that trade liberalization *alone* is not likely to be enough to generate economic growth over the ensuing six years. In fact if anything it seems to have a *negative* albeit often statistically insignificant impact, on average, for at least the first three years after the reform. Dropping down to the level of The World Bank income (WBI) groups, we find that this result is largely borne out—for example the effect is quite negative for high-income countries regardless of the length of time after the reform. The one exception is the upper-middle income group, which would experience a contemporaneous improvement in the period of the reform but not thereafter. We then present analysis to suggest a more nuanced view, namely, that if a country's institutions of law and order exceed a certain level of adequacy (that is, the thresholds mentioned above) then trade liberalization can indeed lead to gains in economic performance. While a trade liberalization in 1991 would have likely led 19 out of 31 low-income countries to experience losses in economic performance by 1997, a trade liberalization in 1997 would have brought, because of the improvements in law and order over the intervening 6 years, macroeconomic gains to 18 out of 31 countries and with *none* of the group likely to experience a loss. Nonetheless, even allowing for institution quality, we still find that trade liberalization can have a negative economic impact for some countries. For example, based on thresholds computed for the full sample, any country whose quality of law and order is a bit worse than one-and-a-half standard deviations below the mean, would likely experience a negative economic impact in the year of the liberalization. The paper computes more precise thresholds (both for likely gains as well as losses) by income group and indicates which countries would fall into each case. In sum, one size policy does *not* fit all and its impact depends on initial conditions–and on the quality of supporting institutions in particular.

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1 Introduction

How much reform can a country take and still have a positive impact economic growth? As veteran reformers always suspected and unlike the dictum of Economics 101, "more is not necessarily better". For as many as there are reform success stories, we find an equal number in which the impact of reform has been at best neutral and in many cases discernibly negative. A concrete example is privatization in the former centrally planned economies. According to a recent USAID study (Zinnes et al. 1999), the macroeconomic impact of privatization was not uniformly positive. It turned out that over the decade of the 1990s, only where the underlying supporting institutions were of adequate quality was privatization growth-enhancing.

In the present paper we build on this insight and propose a method for USAID to gauge the likely impact at the macroeconomic level of further reform, i.e., whether more reform would likely be growth enhancing as measured by increased national income. The method is based on an evaluation of the combined past reform experience of countries for the sector under consideration. We do this by creating an indicator of policy, computed as the (standardized variance of the) unexplained performance of the sector under analysis once non-discretionary characteristics (e.g., culture, geography, climate, quality of rule of law) are purged. We then use the indicator to benchmark countries over time, both compared to all countries as well as to the relevant country cluster. Then, by analyzing country scores relative to macroeconomic performance, we are able to compute country-specific thresholds, based on a country's institutional characteristics that indicate when further reform is likely to improve, worsen, or have uncertain effect on macroeconomic performance. Since reforms can have different short-run (contemporaneous) and long-run impacts, we also carry out the analysis for impacts three, six and twelve years into the future.

* The authors express their thanks to Steve Hadley, Steve Knack, Fred Witthans, the USAID Forums Steering Committee, and to Omar Azfar. All errors remain with the authors.

These country benchmarks and thresholds, which one can think of as "rules of thumb", may serve several useful functions for USAID. First, they provide a starting point for discussion on whether a country should implement further reform in the particular area when other, more detailed, studies are unavailable. (In fact, even when such studies are available, they tend to be country-specific and use different methodologies, making comparisons difficult.) Our indicator should therefore be helpful for mission directors either who have just arrived in-country or who want an independent, comparative assessment for a new sector. Second, the indicator should be a helpful orienting device for USAID/Washington staff who participate in parameter-setting meetings on diverse countries and sectors and lack the time for extensive preparation. Here the indicator provides an independent–and cost-effective–reference point with which to evaluate the economic assessment or proposals in the country assistance strategy document. A country well below its threshold should exhibit positive idiosyncrasies that compensate for apparent inadequacies relative to other countries in its group. However, the operative term here is "orientating". While the indicator describes the expected performance based on countries with a similar set of initial conditions, the indicator is not an alternative to fielding a sector preparation team.

To motivate the problem to which our diagnostic methodology provides a solution, we begin in Section 2 with the examples from privatization and trade liberalization. The rest of the paper lays out the five steps required to produce a diagnostic toolkit, illustrating the process for the case of trade liberalization. Section 3 describes the general methodology which we then utilize to construct an application for the case of trade liberalization. Section 4 presents a summary of the results of our empirical analysis of the trade liberalization application. Using these to determine whether a country exceeds the thresholds required for additional trade liberalization to be growth- (income-) enhancing, we develop in Section 5 a practical toolkit for use by USAID (without the need of outside experts) using these thresholds. We end in Section 6 by offering a series of conclusions, including suggestions for future applications.[1]

2 Two examples where "more may not be better"

Let us consider two examples of where it might be helpful to have some rules of thumb to help identify opportunities or dangers for economic reform. Section 2.1 presents the case of privatiza-

[1] The interested reader is directed to Zinnes and Subrick (2003, forthcoming) for a more extended though technical presentation of methodology applied to the case of trade liberalization.

tion, drawing from the work of Zinnes, Eilat and Sachs (2001). In Section 2.2 we consider the current debate on whether it is wise for a particular country to undergo further trade liberalization. As is often the case in such controversies, we speculate that both sides may be "right"–depending on the initial conditions. We suggest–and confirm in the second half of the paper–a more nuanced view. For the case of privatization a country's supporting institutions must exceed a certain threshold level of adequacy in order for additional privatization to bring economic growth. Similarly, if a country's institutions exceed a certain level of adequacy then trade liberalization can indeed lead to gains in economic performance. In both cases, if the quality of institutions falls below the threshold then a good rule of thumb is that the reform will lead to macroeconomic losses, for example, persistent unemployment.

2.1 The gains from privatization

For our first example, we consider the debate over whether "more privatization is better". In other words, does further reform–such as privatization–necessarily enhance economic growth? To address this question, we draw upon the work of Zinnes, Eilat and Sachs (2001). They evaluate the first decade of economic reform in transition economies to clarify what factors contributed to the gains from privatization in transition economies. For many years, they note, the policy makers' views on privatization reflected the "Washington Consensus", which equated change-of-title (COT) with privatization. As a result, COT became *the* policy imperative.

Based on a review of the literature on the gains from privatization, however, Zinnes *et al.* identify the importance of additional factors (which they refer to as "OBCA" reforms). These include the institutions and regulatory framework to address depolitization of firm management [O]bjectives, hardening [B]udget [C]onstraints, and [A]gency (incentive and contracting) issues. The OBCA reforms also may be thought of those directed at prudential regulation, corporate governance, and developing capital markets. They then examine the empirical evidence across 24 countries to determine whether COT alone has been sufficient to achieve economic performance gains or whether these other "OBCA" prerequisites are important.

They first arrange the transition countries into a typology of countries clusters with similar initial conditions for economic performance. Then they develop an indicator to capture the degree of change-of-title (COT) privatization and one to capture how a country's regulatory institutions address the OBCA issues.

Armed with these institutional indicators, they first econometrically show that privatization involving change-of-title alone did not generate economic performance improvements. This result is robust to the several alternative measures of economic performance, including GDP recovery, foreign direct investment, and exports. They then introduce their OBCA indicator. They find that, while the COT and OBCA measures on their own contribute to economic performance improvements, the real gains to privatization come from complementing (combining) change-of-title reforms with OBCA reforms. As Pistor (2001) underscores, it is only when the legal and regulatory institutions supporting ownership are in place and functioning that owners can exercise their prerogatives conferred by a change-of-title to pressure firms to improve their productivity and profitability. Only then will the economic performance of the country improve, too.

Zinnes *et al.* go on to show that under certain conditions these results need to be qualified in two ways. First they find that the higher the level of OBCA, the more positive the economic performance impact from an increase in COT privatization. In particular, where COT has a positive impact, the impact will be even more positive the higher is the level of OBCA; where COT has a negative impact, the impact will be *less* negative the higher is the level of OBCA.

A corollary to this result is that there is a threshold level of OBCA in order for change-of-title privatization to have a *positive* economic performance response. Thus, if complementary OBCA reforms are not sufficiently developed, change-of-title privatization may have a *negative* performance impact. An explanation for the cases of worsening overall economic performance from COT privatization is that transfer of ownership without the institutional structures in place for owners to exercise their authority simply replaces poor government control of management with weak or no private sector control. Zinnes *et al.* also find that the corollary's obverse is true: an improvement of OBCA does not guarantee economic performance improvements unless a minimum (threshold) level of change-of-title privatization has already been attained. An explanation for this may be that reforms that harden budget constraints but do not transfer control to private (and, therefore, profit-maximizing) owners may hurt economic performance. Fortunately, their analysis shows that the threshold COT level for this worrying effect is quite low, with all the countries in the affected clusters well above it by the end of the decade.

The second qualification Zinnes *et al.* makes is that the economic performance responses from COT privatization are sensitive to the cluster carrying out the policy. The economic per-

formance response to change-of-title privatization was in general significantly positive for the EU Border States and the Baltics, negative for the Western FSU, and ineffectual in the Balkans, the Caucasus, and Central Asia. Increases in OBCA led to performance improvements in the EU Border States, the Baltics, the Caucasus, and Central Asia and led to performance losses in the Western FSU and the Balkans. In short, "one size (policy) does *not* fit all"; privatization policies must be tailored to the (cluster-specific) level of complementary reforms in place.

The Zinnes *et al.* paper allows policy makers as well as donor technical assistance providers to draw two main recommendations. First and foremost, they should consider carefully when recommending quick privatization if the requisite OBCA-related, legal, and regulatory institutions are not in place and functioning. Economic performance gains come only from "deep" privatization, i.e. where change-of-title reforms occur in the presence of high enough levels of OBCA. Second, the idea of "one size fits all", at least from the policy perspective, does *not* apply to transition countries. As a result of different initial conditions, the economic performance responses of countries to the *same* policies are different. In the area of privatization, these responses depend on the level of complementary reforms–and on OBCA-related reforms in particular. Policy prescriptions, therefore, should be less ideological and more tailored to the country's institutional conditions and stage of transition.

Zinnes *et al.* end by concluding that a new privatization paradigm has emerged: "While ownership matters, institutions matter just as much".

2.2 The gains from trade liberalization[2]

What does it mean for a country to benefit from trade liberalization? The literature (Krueger 1978; Bhagwati 1978) provides diverse hopes for these benefits. These include faster and more prolonged GDP growth leading to a higher standard of living, a greater stake in world peace, greater democracy, and more empowered civil society. On the other hand, the same literature (Daly 1994; Rodrik 1999, [Guy against Easterly in debates 2001]) provides a litany of negative impacts to unsuccessful liberalizers. These include extensive unemployment, disruption of rural communities hitherto dependent on import-substituting firms or subsistence agriculture, loss of domestic "sovereignty", uncontrolled urban congestion and stress on municipal infrastructure,

[2] This subsection draws heavily from Zinnes and Subrick. (2003, forthcoming).

uneconomic extraction of its natural resource base, and increased volatility of the macro-economy, to mention but a few.

Whether a country is ready for further trade liberalization, therefore, depends on whether the benefits exceed the (presumably short-term) costs and whether ways can be found either to reduce the number of losers or to efficiently compensate them, so as to address equity concerns. And losers there must be *by necessity* since the efficiency gains from trade liberalization depend in part on the country succeeding in reallocating resources away from its now less competitive sectors (or products) and to those where it has a comparative advantage. The ability of the country's economic agents to respond flexibly to new the new opportunities and the degree to which the losers can be compensated depends on the quality of the country's supporting institutions. Worse, since the losers (and even the winners) are uncertain who they are and there is general skepticism about *future* compensation schemes, garnering the political will to liberalize may require signaling credible commitment regarding the government's intentions (Fernandez and Rodrik 1991, Dixit and Longredan 1995). This again depends on the quality of the country's supporting institutions. Better institutions provide the basis for credibly committing to various forms of social insurance that increases the likelihood that trade liberalization will occur.

From a foreign policy perspective, the United States may find failed trade liberalization worse than none at all. Failure in one country creates an externality by making liberalization appear more risky and therefore less likely to be adopted elsewhere. Fewer countries open their markets to U.S. goods and services and U.S. products become less likely to be bought and sold in foreign markets thus reducing the demand for U.S. goods. Of course, trade liberalization is not an all-or-nothing policy. The key is to match the degree of trade liberalization with the characteristics of the country's institutions. That is, to promote trade liberalization once a country attains a minimum institutional threshold that secures the institutions of private property that attract international traders. These institutions include a democratic political regime, an efficient legal system, and other formal and informal organizations that prevent the divergence of private and social costs. If the institutions that protect property are strong, then an ambitious liberalization may be the best course of action. If, on the other hand, a country has weak institutions, limited trade liberalization may be in order. A USAID mission may wish to have a tool comprising a set of benchmarks with which to determine whether key institutional thresholds have been met,

given the nature of the country's trade regime. Such a tool could assist the USAID mission in preparing its country overview.

With a view to putting into context our own contributions below, let us underscore some of the salient conclusions of the trade liberalization literature, prior to developing such a diagnostic tool.

The relationship between economic growth and openness to international trade is, in general, positive (Harrison 1996). Those countries that adopt policies that encourage international trade are also countries that exhibit economic growth (Sachs and Warner 1995, Frankel and Romer 1998). Openness to international markets improves domestic market competitiveness, extends the division of labor, and allows countries to take advantage of their comparative advantage.

Early empirical evidence found a positive relationship between openness to international trade and economic growth. Two of the most well-known papers were by Michaely (1977) and Belassa (1978). They found that countries that adopted export-oriented policies had better growth performances than countries whose policies that favored import-substitution policies. Bhagwati (1978) and Krueger (1978), each based on study by the NBER, argued that openness provided additional evidence that international trade increased economic growth. Rather than use a large country sample, they examined in detail the effects of trade liberalization on 10 developing countries. They identified when countries policies became more open and when they became less open. If a country liberalized its trade policies, then the country experienced higher economic growth.

More recently, a number of papers have provided additional evidence that openness to international trade improves economic growth. Dollar (1992) proposed an alternative measure of trade openness based in exchange rate distortions due to trade policy. He found that from 1975-1985, distortions to exchange rates were associated with worse economic performance.

Edwards (1992, 1998) develops a simple model of the effect of trade liberalization on economic growth. He examines the robustness of the relationship between trade openness and economic growth. Using nine different measures of openness and various econometric tech-

niques, he finds that the relationship between trade openness and growth is positive and statistically significant.[3]

Sachs and Warner (1995) construct their own measure of openness to international trade. It has five components. They are (1) whether or not the country has a socialist economic system, (2) if the average tariff rate exceeded 40 percent, (3) if the black market premium was greater than 20 percent in either the 1970s or 1980s, (4) if non-tariff barriers covered more than 40 percent of imports, and (5) if there was a state monopoly of major exports. They found a robust relationship between their measure of openness and economic growth. They also found that those countries that liberalized their trade regimes tended to converge whereas those countries that were closed to international trade experienced divergence.

Although a large literature has emerged that provides ample evidence that international trade improves economic growth, there are some problems with the empirical literature. The primary problem is that the results tend not to be robust across country samples and time periods. Rodriguez and Rodrik (1999) examined the four most-cited papers in the international trade and development literature and found that each measure of openness is not robust there are changes in the sample size or time period. They found little evidence that policies that promoted international trade were significantly correlated with economic growth. One significant problem according to Rodriguez and Rodrik (1999) is that poor macroeconomic policies are correlated with measures of trade openness. For example, if a country experiences high inflation, then it is also likely there are accompanying macroeconomic problems that complement the trade distorting policies.

Yet it should be noted that they did not claim that there was a negative relationship between economic growth and international trade. In the worst case, there is simply no relationship. This is important to note because even the lack of robustness has never suggested that openness to international trade slows economic growth. Rather, openness may influence growth and development through other channels, such as the promotion of good institutions that support development. These indirect channels of openness on growth may be more important than the direct effect.

[3] He does not claim to have solved the causality problem. He recommends time-series analysis in order to solve that problem.

Harrison (1996) and Pritchett (1996) have raised similar questions. Harrison found only a general tendency in favor of the positive relationship between openness to trade and economic growth. The results were sensitive to econometric technique and how openness was measured. Pritchett (1996) finds that the various measures of trade policy-trade share of GDP, average tariff rate, deviations of actual trade patterns from predicted trade patterns, and price distortions- are pair-wise uncorrelated. That is, the various measures of trade policy are completely uncorrelated across countries.

Institutions, too, have been found to have a positive impact on economic growth (Knack and Keefer 1995). Most studies include a measure of the protection of property rights as a proxy for institutional development. Knack and Keefer (1995), Acemoglu, Johnson, and Robinson (2001) and Hall and Jones (1999) found that the protection of private property has a significant influence on economic development. The literature on the relationship between international trade and economic growth, however, has not explored the institutional foundations of international trade. A good example of this is the recent article by Wacziarg (2001). He develops a dynamic model of how trade policy might affect growth. The three channels he identifies are factor accumulation, transmission of technology, and allocative efficiency. Institutions are *not* one of his channels.

One partial exception on the empirical side is Wei (1999), who examines the impact of natural openness on institutions. He argues that those countries that are natural open – access to major waterways, close to major trading partners, small in geographic size – face higher opportunity costs to develop high-quality institutions that promote international trade. If they do not have high-quality institutions, they engage in less international trade, the division if labor is limited, and incomes fall. As will be seen, our model is in this spirit.

While enormous energies were devoted to the political economy of international trade policy in the 1980s (Baldwin 1988; Rodrik 1995), recently, Rodrik (2000) asked the provocative question of whether or not markets can become international while politics remains local. That is, they ask what the political institutional foundations are of international trade. Borders between countries demarcate both legal and political jurisdictions. Both the political and legal system influences the amount of transaction costs. As a result, trade across countries is substantially influenced by national borders. For example, McCallum (1995) provides evidence that the border between the United States and Canada significantly reduce trade between the two countries.

Obstfeld and Rogoff (2000) also provide a transaction cost explanation for trade between neighboring states with similar factor endowments.

The effect on trade openness on economic growth appears to be positive. But this addresses only one of the concerns regarding the costs of trade liberalization. Incomes may rise but other factors that people value may be harmed. For example, sub-optimal natural resource extraction, increased income inequality, and increased crime may accompany trade liberalization that offsets any gains from higher incomes.

Natural resources and the environment. Trade liberalization does not directly affect the environment. Poor environmental outcomes do not result from opening one's market to foreign competition. Environmental degradation results from poor institutions not from foreign competition. In particular, the lack of protection of both private and common property underlies environmental problems such as pollution. If private and public firms do not bear the total costs of their production decisions, negative externalities result. For example, if firms (either private or public) do not compensate individuals who they harm, the firm produces beyond the optimal level.

Similarly, if the governmental structure does not encourage long-term planning, politicians and regulators will not adopt efficient resource-extraction policies. Reduced time horizons alter the optimal policies and excessive extraction occurs. For example, if a regime believes that it will lose the next election or that their overthrow is likely, then they adopt policies that over-extract resources so that they can profit in the short-run. While natural resource depletion does not directly depend on trade policy, a weak or compromised regulatory structure can be overstressed when trade liberalization increases the demand for extractive resources.

Income inequality. Critics of trade liberalization often claim that openness increases the extent of income inequality within a country. Increasing the number of imports reduces the number of workers in low-skill occupations where wages are low while at the same time increasing exports increases the number of workers in high skill sectors where there are high wages. Income inequality increases. These concerns are ill-founded. First, high wages result from high productivity. High productivity results from human capital investments such as schooling and technology. Each of these is, in turn, the by-product of good institutions that secure the returns to entrepreneurs

Furthermore, little evidence supports the claim that trade openness increases income inequality. Incomes inequality does not appear to change that much over time (Li, Squire, and Zhou 1998). The persistence of income inequality over time suggests that the primary determinants of income inequality are not policies such as free trade but rather the result of fundamental factors such as institutional quality (Chong and Calderon 2000).

To sum up, the oft-mentioned costs of trade liberalization and reform more generally result from institutional deficiencies. The lack of institutions of private property prevents reforms from succeeding due to the increased likelihood of "capture" by adversely affected groups. Recognition of the role of institutional quality in the effectiveness of reforms focuses the problem at hand. Our contribution is in this spirit. We ask a complementary question: what are the institutional pre-requisites for international trade liberalization to be successful? That is, what is the level of institutional development necessary for trade liberalization to lead to growth? We conjecture that opening one's domestic markets to international trade leads to economic growth only when the quality of institutions of private property exceed a certain threshold. Trade liberalization does not improve economic performance when property rights are poorly protected. Insecure property rights discourage international traders. Less investment occurs domestically and less innovation occurs. Growth rates fall.

Good institutions organize the production of infrastructure, urban services, protection of the environment, unemployment insurance as mobile factors need to physically relocate or receive training. Also influencing a country's ability to capture the potential gains from trade is the quality of its regulatory institutions and their promulgation, monitoring, and enforcement of accounting standards, use of collateral registries, and corporate governance oversight, among many others. Finally, weak property rights enforcement will likely reduce the interest of international trader's to trade in that country or raise the transaction costs of doing so. Simply lowering tariffs or removing non-tariff barriers will not lead to growth-enhancing trade openness without the accompanying minimal level of institutions.

Finally, we mention that the search for links between policy and institutions, on the one hand, and economic performance, on the other, has also spawned a cottage industry in the design of indicators for policy and institutions.[4] In fact, from the very beginning, the literature on the

[4] See Knack (2002) for a review of this literature.

impact of trade liberalization has sought indicators capturing the of a trade regime. Balassa (1978) used "phases", Krueger (1978) used a quantitative measure involving exchange rates, and, most recently and in the same tradition, Wei (1999) creates an indicator of "natural openness". In addition to individual scholars, there are several organizations among those whose indicators are frequently used, including Freedom House[5], Heritage Foundation, International Country Risk Group, IRIS, Transparency International, and the World Bank[6].

3 Conceptual methodology

In this section we present the conceptual model upon which our diagnostic tool is based. In the following section we illustrate the tool with an empirical application to trade liberalization. Toward this end, we first develop our basic model linking economic reform, institutions, and country-level economic performance. Then we show how to apply the basic model to a particular policy issue, in this case trade liberalization.

3.1 Rules of thumb on when more reform would be pro-growth

The key feature of our approach is the testable hypothesis that the economic growth benefits of a given degree of policy regime reform depend on the quality of several dimensions of the country's institutions, which we described above. For this purpose, we build upon the framework developed in Zinnes, Eilat and Sachs (2001) for the case of whether privatization is beneficial. Their methodology utilizes cross-country panels to determine whether a country's relevant institutions exceed the thresholds required for additional policy reforms to yield economic performance gains at the level of economy as a whole. In Section 5.3.1 we describe how USAID can use these thresholds (benchmarks) as "rules of thumb" to support various aspects of their programming.

We start by positing the following simple model, $Y=G/B$, of country-level economic performance, Y. Here, $G(Q,P,I,\boldsymbol{X})$ and $B(Q,P,I,\boldsymbol{Z})$ are the good and the bad effects of further policy reform and Q, P, and I are (technology-adjusted) factor inputs, policy, and institution quality and $\boldsymbol{X}$ and $\boldsymbol{Z}$ are vectors of other pertinent variables specific to G and B, respectively. To accurately

[5] In fairness, Freedom House's indicators pre-date the recent wave of interest in institutional and political indicators.
[6] The World Bank has had a leading role in this industry. See Kaufmann *et al.* (1998) as well as Beck *et al.* (2001) for the World Bank's newest dataset on these topics.

capture the degree of policy reform, P here refers to the realization of the policy regime, including its legal, regulatory and administrative apparatus and not simply its *de jure* legislative status.

We then consider the following specification

$$G = D\, I^b\, Q^a\, P^{N(I)} \quad \text{and} \quad B = A\, Q^d\, P^{M(I)}\, I^c \tag{1}$$

where N and M are functions, a, b, c and d are a coefficients, and A and D country-specific constants capturing the $\boldsymbol{X}$ and $\boldsymbol{Z}$. Thus

$$\text{Ln}\, G = \text{Ln}D + a\, \text{Ln}\, Q + b\, \text{Ln}\, I + N(I)\, \text{Ln}\, P \quad \text{and} \quad \text{Ln}\, B = \text{Ln}A + d\, \text{Ln}\, Q + M(I)\, \text{Ln}\, P + c\, \text{Ln}I$$

Here we would expect a, b and d to be positive, c to be negative and N and M to be positive, real-valued functions.

Next, we down-play real effects by making some simplifying assumptions regarding the (real) production function. First, we assume that the output elasticity of technology-embodied inputs, a-d, is unity.[7] We then approximate technology-embodied factor inputs by $Q = \mu kL$, where L is the size of the population, k is the share of the labor force in the population, and μ is the marginal productivity of labor. Using the equations (1) and the definition of $Y=G/B$, we may rewrite log output as

$$\begin{aligned} \text{Ln}Y &= [\text{Ln}D - \text{Ln}A + (a\text{-}d)\text{Ln}(k)] + (b\text{-c})\text{Ln}\, I + [N(I) - M(I)]\text{Ln}\, P + (a\text{-}d)\text{Ln}(\mu L) \\ &\equiv \qquad K \qquad\qquad + e\, \text{Ln}\, I \qquad + \Omega\,(I)\, \text{Ln}\, P \qquad\qquad + \text{Ln}(\mu L) \end{aligned}$$

Finally, replacing $\Omega\,(I)$ by its first-order (linear) approximation, $n + m\, I$, where n and m are coefficients and using our factor input assumptions, then

$$\text{Ln}[Y/(\mu L)] = K + n\, \text{Ln}\, P + b\, \text{Ln}\, I + m\, I\ \text{Ln}\, P \tag{2}$$

To consider the effect of a change in policy regime on country-level economic performance, we differentiate this expression with respect to P, yielding

$$\{\partial \text{Ln}[Y/(\mu L)]\}/\partial \text{Ln}P = n + m\, I \tag{3}$$

[7] While this is a rather strong assumption, we do not believe that the errors it introduces are correlated with $\Omega\,(I)$. In other words, the simplification should not bias our hypothesis tests below. There are several ways to address this simplification in a future version of the diagnostic instrument.

Thus, in order for further policy reform to have a positive effect this last expression must be strictly positive. This requires that $I > -n/m$ in order that a reform in the policy regime have a positive effect; otherwise its effect is anti-growth.

This model allows us to test the importance of institutional quality for the effectiveness of a policy change on economic performance. First, we would expect that the quality of institutions would have a positive impact on economic growth. Second, the sign of the direct policy effect is uncertain and possibly statistically insignificant but is likely to be small in magnitude. Third and most critically, we expect that the interaction term between the policy regime and institutions to exert a large impact on economic performance.

3.2 An example: rules of thumb for pro-growth trade liberalization

Let us consider as an example the case of trade liberalization to illustrate how the above model might be adapted for a particular policy issue. As discussed in Section 2.2 among the central concern raised in the debate for and against trade liberalization is that, while it may in theory be a good thing, many countries are not ready for it. Among the reasons given for this lack of readiness include (i) the administration of social safety nets are inadequate at best to deal with the displacement caused by resource reallocation, (ii) the regulatory institutions related to natural resources and the environment as well as urban amenities are simply too weak to confront the market forces unleashed, (iii) alternative fiscal systems are unavailable to replace lost revenues from trade taxes, (iv) local capital markets cannot provide local firms–and especially the SMEs–the financing for responding to the increased competition from imports.[8] The debate, then, seems to revolve around whether there are thresholds for the quality of institutions in order for further trade liberalization to produce gains in economic performance at the country level.

The diagnostic toolkit which USAID field missions could use requires four steps to develop. First, the conceptual model above must be adapted to the policy under consideration, trade liberalization in this case. Second, one must develop a statistical measure of trade liberalization. For many policies USAID will find such variables are readily available. Nonetheless, to provide a comprehensive example of our diagnostic tool, we illustrate how USAID may itself (or, rather, its consultants) develop a suitable policy measure across countries and time. The third

[8] Presumably this last one is a short-term concern since, in principle, the real exchange rate should eventually adjust raising the relative price of imports to domestic goods.

step is to estimate the model using the so constructed policy measure. Finally, one calculates the pro-growth policy thresholds from the estimated coefficients. We illustrate Steps 1 and 2 now and Steps 3 and 4 in Section 4. Then in section 5.3 we present and apply the diagnostic toolkit derived from these steps.

Step 1: Adapting the basic model. In order to determine the threshold level of institutional development necessary for trade liberalization to succeed, we begin by examining the effect of openness and institutional development on a country's level of economic activity, *Y*. Building upon the discussion of Section 2.2 we posit two effects from openness.[9] First, trade brings efficiency gains through competition and lower prices. Second, the strength of this effect depends on the quality of the supporting institutions (e.g., marketing and distribution, enforcement of contracts and other transaction cost-reducing institutions). Third, output growth resulting from increased trade can also bring increases in pollution, urban congestion and crime from unemployment caused by worker displacement. These negative externality effects are often of a common property nature and require collective action through institutions to control. The extent of these problems depends on the quality of the country's institutions. High-quality institutions alleviate the problems of urban congestion, pollution, and crime.

We then can rewrite equation (1) as

$$G = D\, I^{b}\, Q^{a}\, T^{N(I)} \quad \text{and} \quad B = A\, Q^{d}\, T^{M(I)}\, I^{c} \tag{4}$$

and equation (2) as[10]

$$\text{Ln}(Y/L) \;=\; K + \; n\,\text{Ln}\,T \; + \; b\,\text{Ln}\,I \; + \; m\,I \;\,\text{Ln}\,T \tag{5}$$

To consider the effect of a trade liberalization we differentiate this expression with respect to *T*, yielding

$$(\partial \text{Ln}Y/L)/\partial \text{Ln}T \;=\; n + m\,I \tag{6}$$

Thus, in order for trade liberalization to have a positive effect this last expression must be strictly positive. This requires that $I > -n/m$ before trade liberalization has a positive effect; otherwise its effect hurts economic performance. Our hypothesis to test, therefore, is first whether the direct trade effect, whose sign of is uncertain theoretically, is either statistically insignificant or small in

[9] Here we ignore other dynamic effects on civil liberty and democratization.

[10] Here we have set μ to 1. Zinnes and Subrick (2003) examines the results for the cases where we use several alternative proxies for μ, including number of Internet server sites and the morbidity rate.

magnitude.[11] The second is whether the interaction term between openness and institutions exerts a large impact on economic performance, as our theory suggests it should.

Step 2: Constructing a policy measure. As discussed above, the degree of trade liberalization depends upon the *de facto* level of trade policy, which we refer to as the trade policy regime. Unfortunately, the trade policy regime is not straight-forward to measure. What one observes is the equilibrium *outcome from* the regime, i.e., the amount of trade resulting from the interaction of the supply and demand side for domestic and foreign goods as filtered through the trade policy regime, on the one hand, and the quality of the policy regime due to the political-economic pressure generated by the interaction of supply and demand for domestic and foreign goods, on the other.

In order to capture the effects of the trade policy regime, we formulate the following model. We start by assuming that the amount of trade in equilibrium is the result of a country's intrinsic geographic (g) and cultural (k) factors and the "gravity"[12] (v) of attraction between it and its possible trading partners.[13] To this we add two, additional, anthropomorphic terms, whose need by now is obvious. The first is the country's trade policy regime (T) and the second is the quality of the country's institutions (I), both as described in the basic theoretical model above.

Let r be a measure of the intensiveness of a country's trade, often called "openness". Then we may use the variables mentioned in the previous paragraph to heuristically represent this measure as $r = \Phi(g,k,v,T,I)$, where $\Phi(.)$ may be thought of as the trade "production function". We assume that Φ is such that T is at least multiplicatively separable in the following way:

$$r = \Phi(g,k,v,T,I) = F(g,k,v,I)\, H(T) \tag{7}$$

where F and H are functions. Noting that by assumption r is observable (e.g., the share of trade in GDP), then we may then recover a measure of the trade policy regime by rearranging these terms to yield

$$H(T) = r / F(g,k,v,I) \tag{8}$$

Finally, we may rewrite the trade production function in equation 7 in log-log terms as

$$\text{Ln}\, r_{it} = \rho_0 + \rho_1 \text{Ln}\, g_{it} + \rho_2 \text{Ln}\, v_{it} + \rho_3 \text{Ln}\, k_{it} + \rho_4 \text{Ln}\, I_{it} + \varepsilon_{it} \tag{9}$$

[11] Again, this is because it would be capturing two effects of opposing sign.
[12] Gravity models have been highly successful in predicting trade flows. See Wascrzig (2001).
[13] Wei (1999) has a similar model in mind when defining his "natural openness".

where ε_{it} is the regression error term and ρ_j, j=[1,4] are the coefficients to estimate.

4 Empirical application to trade liberalization

In this section we illustrate the application of our diagnostic tool by working through the steps 3 and 4–the empirical side–of the application described at the outset of Section 3.2 above.

4.1 Step 3: Estimation of the model

Using readily available cross-country data sources[14] in Step 3 we may readily estimate the two-stage, cross-country regression model comprising equations (5) and (7) developed in Section 3. In the first stage, we estimate an indicator for a country's trade policy regime. We use this in a second-stage estimation to investigate the effect on economic performance of the interaction between institution quality and the estimated trade policy regime.

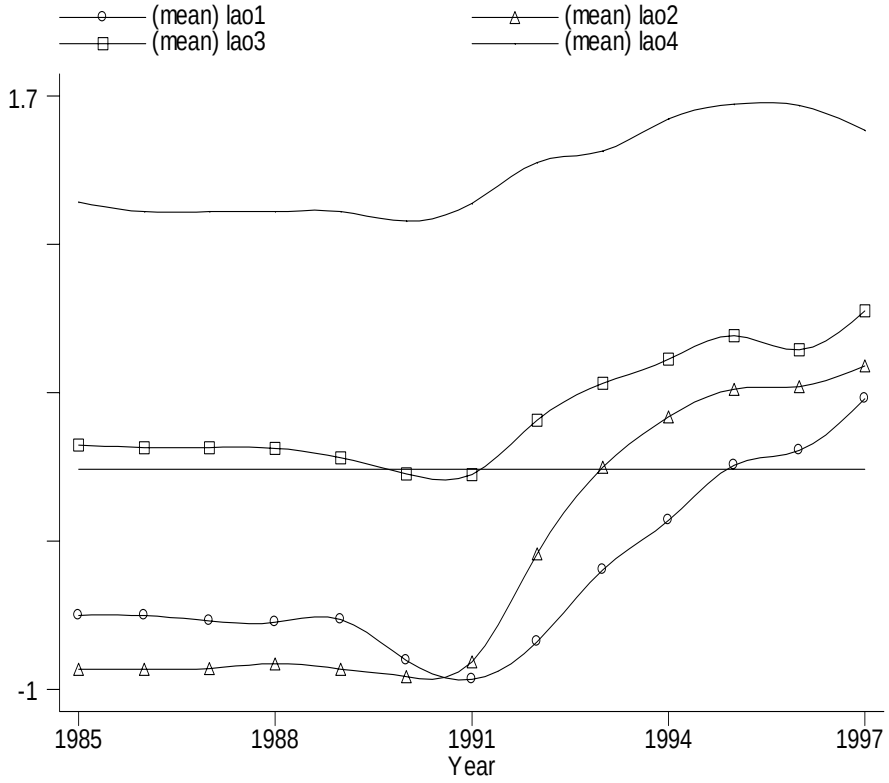
4.1.1 Stage-I regression: Trade policy regime indicator

In this first stage, we estimate an indicator for a country's trade policy regime. We start by choosing proxy variables in the order they appear in equation (9) for each of the influences described in section 3.2. We then estimate equation (7) and calculate our trade policy regime indicator according to equation (8) as the difference between the actual value and the predicted value of country openness.

Consider *g*, the geographic variable and *v*, the gravity variable. Larger countries are less likely to engage in international trade because they have their own larger internal markets. This reduces the costs of not engaging in international trade. We proxy this effect by Ln(AREA), the country's total surface area. Geographic factors influence, *ceteris paribus*, the difficulty of engaging in international trade. We include therefore a dummy variable, LLOCK, if the country is landlocked, a dummy variable ISLAND, if the country is an island, and a variable for the distance to one of five major international ports[15]. Finally, if a country does not have access to navigable waterways, it is less likely to engage in international trade. Major waterways reduce the costs to engage in international trade. Thus, we include COAST, the percentage of land than is within 100 kilometers of a major waterway.

[14] See Appendix A for the data sources used in this study.
[15] Miami, Los Angeles, Hong Kong, Rotterdam, Tokyo.

Figure 1: The evolution of law and order (LAO) by income group, 1985-1997.



Notes: Here, lao1 to lao4 refer to the average value of LAO over each of the four income groups of Table 6, with 1 being the low-income group and 4 being the high-income group.
Source: LAO is produced by ICRG (see Table 5).

The principal culture variables available are religion and linguistic group. We include a dummy variable if the official language is English, to reflect the fact that English is both the language of international commerce (and therefore knowing it reduces the transaction costs to trade) and increases access to the media containing marketing and technology information as well as materials on modern business methods.[16] We also include dummy variable for Spanish and French speaking countries.

The structure of the economy may exert an impact on international trade. For example, societies that are primarily agriculture adopt different trade policies than countries which are primarily manufactures. Thus, we include AG which is the percentage agriculture is of GDP.

[16] Wei (1999) also finds that English facilitates trade.

Finally, we include our measure of institutions. In order to measure institutional quality, we use the International Country Risk Guide's measure of law and order, LAO.[17] This serves as a proxy for the quality of the institutions that protect private property. We feel this is a satisfactory measure of institutional quality. Historical studies (North and Thomas 1973) and more recent cross-country empirical work (Knack and Keefer 1995; Hall and Jones 1999) have found the protection of private property to be strongly correlated with economic performance. LAO is measured from 1 to 6 with higher numbers being associated with better protection of property rights. Figure 1 summarizes the evolution of this indicator over the sample's time period for each of the four World Bank income groups.

Turning to the left-hand-side variable, country "openness", *r*, we take the most common measure of in the literature, the ratio of the sum of imports plus exports to GDP.

We can now estimate regression equation (9) using the proxy variables we have just described. The results are provided in where the dependent variable is the trade share and the columns (1) and (2) cover the full sample of countries and observations with and without (The World Bank) cluster controls. The gravity variables all have signs predicted by the theory above. Size, law and order, landlocked, tropical, agricultural share of output all increase the expected trade share while distance to foreign markets, manufacturing share of output and whether an oil exporter all lead to a reduced, expected trade share.

We may use the estimated version of equation (9) to "predict" the level of openness that we expect a country to have is based on its geography, cultural characteristics, and quality of institutions. To compute our trade policy regime indicator, we need simply take the difference between the actual and expected trade share of GDP (our degree of country openness proxy). Intuitively, this is saying that the intensiveness of a country's trade is determined by its "natural" openness, its economic structure, and its trade policy regime.

From an econometric standpoint, our trade policy regime variable, TPR, measures the deviation of observed trade shares from the predicted trade shares. Here, we predicted trade shares using a modified gravity equation that includes geographic, cultural, structural, and institutional factors generated TPR (Frankel and Romer 1999). A positive value of TPR—meaning that the actual, observed trade share is greater than what we predicted it to be—implies that a

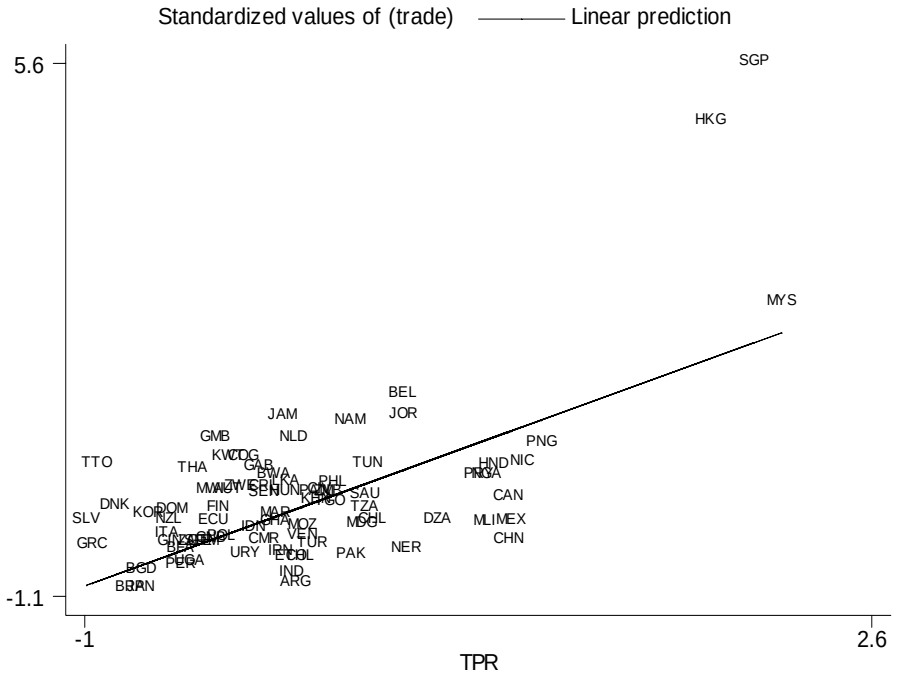
[17] As an alternative institutional variable, we use a measure of executive constraints provided by the Polity IV dataset. The results are similar. See Zinnes and Subrick (2003, forthcoming)

country adopted policies that are more encouraging to international trade than the polices of other countries with similar geographic, structural, cultural and institutional characteristics. Negative values—actual trade shares smaller than predicted trade shares–imply that the country adopted trade policies more *dis*couraging than would have been expected based on other countries' performance with similar characteristics. But what does our measure *really* capture? That is, just how well does it measure what we think of intuitively as the trade policy regime? We address this question in several ways.

First, we examine whether or not our measure of TPR tracks country behavior inter-temporally and cross-sectionally in a way consistent with the traditional measures in the trade policy literature. One measure of trade openness is import duties as a percentage of imports. We use this measure because of its availability over time and a large number of countries. Other tariff measures would severely constrain the number of observations for the calculation. Here, our measure of trade policy regime is significantly negatively correlated (at the 10-percent level) with import duties as a percentage of imports. This provides evidence that our measure does capture one important aspect of what is generally considered to reflect trade policy. As another similar check on the viability of our measure, we examine its correlation with the most common measure of trade policy—the ratio of the sum of exports and imports to GDP. Our trade measure is correlated at the 1-percent level with this ratio. This is illustrated in Figure 2.

As two further examples of how our TPR measure conforms to traditional views of trade openness found in the trade policy literature, Table 1 indicates the countries that score highest, lowest, and average according to our TPR indicator. First, table confirms that TPR conforms to the views commonly held about the countries with the best-performing trade regimes, namely, Singapore and Hong Kong) and the worst-performing, namely China (and Japan among the OECD). It also certainly jives with the literature's views on countries whose trade stance has improved the most, namely, Uganda and El Salvador globally and Ghana South Africa, South Korea, and Greece in their respective clusters. A similar confirming story applies to countries which have worsen their trade regimes, such as Brazil, Niger, and Canada. Figure 1 offers an illustrative example at the disaggregated level of how TPR tracks for a single country, in this case Argentina. As is seen, our measure tracks the reduction in tariffs in Argentina quite well.

Figure 2: A comparison of trade share of GDP to trade policy indicator (TPR) for 1995



Source: Trade share, The World Bank. TPR, authors' calculations.

Second, is it possible that the TPR variable might be systematically capturing something else instead or in addition to the trade regime? In other words, have we forgotten to remove an important source that influences the variability of a country's trade shares? If so, then there would be an omitted variable bias in the regression equation and our residual, that is TPR, would include factors not typically associated with the trade regime. This is a hard criticism to dispute definitively other than to purge all possible other potential influences of the trade share, which is impossible. What we can do instead is to ask what other systematic influences used in the literature might reasonably have been overlooked. One is whether a country is major natural resource exporter—besides oil, which is explicitly controlled for—say, of phosphates, copper, etc., which would cause its shares to be systematically higher than otherwise. To test this, we added to our Stage I regressions, The World Bank Development Indicators' variable "Minerals Exports as a Percent of GDP". This variable was insignificant and added no explanatory power to Stage I.

Third, we consider whether there is something a trade policy regime variable *should* be capturing but ours is somehow missing. The most obvious examples would be overvalued exchange rates and excessive tariff and non-tariff barriers. As a simple check, we compute the

correlation of TPR by income group against several well known indicators found in the literature. These are presented in Table 7. The two indicators of exchange rate appropriateness for an open trade regime, the Dollar index (the level of exchange rate overvaluation) and the black market premium (both from The World Bank's Global Development Network Growth Database) indicate a weak or no correlation with TPR. The two direct measures of a government's interest in free trade—average tariff levels and non-tariff barriers (both from UNCTAD)—indicate a positive correlation with TPR, albeit not statistically significant. Such mixed results, which, in fact, are confirmed by tests in the literature,[18] are also borne out by the other correlations in Table 7: with the exception of the strong and statistically significantly positive correlation between the black market premium and average tariffs, none of the indicators of openness are unambiguously correlated with each other.

4.1.2 Stage-II regression: TPR and economic performance

We now take our TPR series as a proxy for *T* in equation (5) to estimate a second-stage regression to investigate the effect on economic performance of the interaction between institution quality and the estimated trade policy regime.

As our measure of economy-wide economic performance (the dependent variable) we use GDP per capita (GDPPC) measured in constant 1995 U.S. dollars.[19] Given the majority of cross-country regressions use growth rates, let us mention why we choose levels of income rather than growth rates as the dependent variable. The first and most obvious reason is that theoretical model is in terms of income levels—that is, that institutions, as a stock, affect the *amount* of output an economic system can produce—so the arguments for that specification carry through here. Second, our independent variables are measures of the *level* of institutional quality and trade policy regime. Recent literature (Rodrik *et al.* 2002; Pzerorski *et al.* 2000) has begun to advise that using a growth rate as the dependent variable is inappropriate when level variables are the regressors.

We use three control variables in our base regression. The first is country, which captures unchanging characteristics within the country across the time period, e.g., culture or infrastructure, or institutions. The second is year, which captures events in international markets which act

[18] Pritchett (1996) warns that these indicators, though used extensively in the literature, are not themselves highly correlated.

[19] While there may be theoretical arguments regarding the role institutions play for growth, we believe this to be a second-order effect and would require a different model. As such we leave this question outside our current scope.

as a common shock across countries. The third is country cluster, which captures commonalities based on a country's level of development. For this purpose we use The World Bank's classification of income categories (see Table 6).

Our basis regression to be estimated is, as in equation (5),

$$GDPPC_{i,t} = h_1 + h_2 \boldsymbol{K}_{i,t} + h_3 TPR_{,t} + h_4 LAO_{i,t} + h_5 TPR_{,i,t}*LAO_{i,t} + \varepsilon_{i,t}, \quad (10)$$

where $\boldsymbol{K}$ is a set of conditioning variables just described, TPR our measure of the trade policy regime, and LAO our measure of institutional development. The details of the regression are in Table 9.

At the risk of complicated the diagnostic tool, we may both improve its accuracy and realism by making two final adjustments. First, noting that initial conditions across countries and regions are markedly different we ask whether countries should all be lumped into the same single regression model. To test this hypothesis as well as to benefit from the resulting improvement in model specification we can repeat the two stages of the estimation step by country cluster, where each cluster has countries of a similar level of development. We take The World Bank grouping by income to divide our sample into four groups listed in Table 6. These regression resulting are also given in Table 9.

Second, for policy purposes in order to know whether trade liberalization has an impact (or an enhanced one the stronger are the institutions within which it is embedded) we need to consider both short-term *and* longer term effects. For this purpose, we estimate four variations of equation (10). In each one, we consider different lags for the effects including contemporaneous, 3-year lags, 6-year lags, and 12-year lags, i.e.,

$$GDPPC_{i,t} = h_1 + h_2 \boldsymbol{K}_{i,t} + h_3 TPR_{i,t-v} + h_4 ALAO_{i,(t,v)} + h_5 TPR_{i,t-v}*ALAO_{i,(t,v)} + \varepsilon_{i,t}, \quad (10a)$$

where v is equal to 3, 6, and 12, respectively, and $ALAO_{i,(t,v)}$ is the average value of $LAO_{i,t}$ over the period t-v to t. The regression results are also given in Table 9.

Before utilizing these estimated equations, let us summarize the results from Table 9 related to the two types of impacts of greatest interest. The first is the direct impact of trade liberalization on national output. This is captured by the coefficient of TPR. The second impact relates

to the synergistic effect of a trade regime change as it is facilitated or hindered by its enabling environment (captured here by LAO). This effect is captured by the coefficient of TPR*ALAO.

At the level of the full sample, only the very long-term trade liberalization effects are significant. In particular, while the direct effect of TPR strengthens and becomes more negative as we go out to twelve years the effects only becomes statistically significant by year twelve. On the other hand, by year twelve, the complementary effect of liberalization with better institutions has also strengthened and still dominates the direct effect. For the low-income country cluster, the contemporaneous direct effect of TPR is positive, reaching a maximum strength of the four intervals in year 3, and becoming insignificant by year six. In the short run (contemporaneous effect), the effect working through a better enabling environment is also positive and significant– but times stronger. There is no complementary effect in year three and no other statistically significant effects for this group. For the lower middle-income country cluster, our analysis does not reveal *any* statistically significant trade liberalization effects for any time period. For the upper-middle-income country and high-income country clusters, there are also no direct effects of TPR for any time period while in year six the effect working through a better enabling environment is positive and significant. Note that synergistic effects of a better enabling environment on trade liberalization are greatest for the lowest and highest income groups. Finally, below we shall return to the unexpected result of why the synergistic effects of a better enabling environment might be negative, as they are in some of the country clusters.

To check the robustness of this result we repeated the regressions for various specifications and methods. These include using log and non-log specifications, random effects and OLS models.[20] In all cases the coefficient on the interaction term generally remains significant.

4.2 Step 4: Computing the thresholds

Among the consequential results of these regressions is the powerful role of institutions in support of economic performance improvements brought on by trade liberalization. This synergistic effect is captured in the (TPR)*(LAO) interaction term, which, as we saw above is significant in many of the regressions. Of course, we expected this result. There are two aspects of this result which need clarification.

[20] We performed robustness tests using alternative lag structures, calendar year dummies inclusion of quadratic terms (i.e. LAO squared, TPR squared), dividing the sample into sub-samples by period, by income, and by geography, and replacing the country dummies by cluster dummies.

First, we expected that the coefficient on the cross-term would be positive since we believe that the higher the quality of a country's institutions, the more positive is the impact of further trade liberalization on economic performance. That is, if the impact of further trade liberalization is positive, it will be even stronger when institution quality is higher, and if the impact of further trade liberalization is negative, it will be less negative the better are a country's institutions. For the low-income countries, however, better institutions had a *dampening* effect on trade liberalization. This needs explaining, which we do in the next section.

The second clarification relates to the fact that since the effect of a trade liberalization comprises both a direct and an institution-moderated effect, the net of effect is the sum of the two. To understand and assess this net effect, we differentiate equation (10) with respect to TPR:

$$\partial \text{GDPPC}_{i,t} / \partial \text{TPR}_{i,t} = h_1 + h_4 \text{LAO}_{,t} \qquad (11)$$

As can be seen, this equation is analogous to equation (3) in Section 3.1. Equation (11) shows that if h_4 is positive, the higher is LAO, the larger is the effect of a change in TPR on performance. This equation also allows us to determine the level of LAO needed for an increase in TPR to generate a positive performance effect. Note that by construction the sample mean (across all countries and years) of LAO and TP is zero. Consequently, in equation (10) where the coefficient on TPR (h_1) is not significantly different from zero, an average level of LAO is *not* enough to ensure TPR has a positive economic performance gain.

To be more precise about the effect of TPR on performance, we can use direct statistical tests to determine the critical levels of LAO above (below) which an increase in TPR guarantees a positive (negative) effect on performance. We do this by performing one-sided F-tests using the coefficients estimated in regression (11). To find the upper (lower) critical value, we search for the minimum (maximum) value of LAO for which the null hypothesis that in equation (11) dGDPPC/dTPR is smaller (greater) than zero can be rejected for a chosen confidence level.[21]

These results were computed using our full sample of 80 countries. There is a considerable literature that suggests that countries at different levels of development behave quite differently to changes in the policy regime (Zinnes *et al.* 2001; de Melo 1995). As such, we repeat the analysis above for each of The World Bank income groups listed in Table 6. The regressions are

[21] Note that it is possible for a threshold to occur at positive or negative infinity. The theoretical meaning of this is discussed in Zinnes and Subrick (2003, forthcoming).

provided in Table 9 for the TPR estimates based on the income group-specific regressions of Table 8 Table 2 indicates these thresholds by The World Bank income cluster and is discussed in section 5.3.

5 Step 5: Considerations for toolkit design

The empirics of the previous section provide a powerful set of indicators–country benchmarks and thresholds or "rules of thumb"—which may serve several useful functions for USAID programmatic purposes. In order that the analysis be of practical help to USAID, it must be packaged in a "user friendly" way for ease of use and must also be easily accessible to the missions. Thus, the final step in creating a toolkit is to design a "front end" to the empirical analysis. We begin by summarizing the expected purpose of the toolkit prior to describing its components. We then illustrate what the toolkit would look like and how to use it. We then apply it to the trade liberalization example developed in section 4. Finally, we briefly describe how the toolkit should be maintained and by who.

5.1 Purpose of the toolkit

Based on our experience with USAID and the studies it finances, we believe that country institutional benchmarks and thresholds, which one can think of as "rules of thumb", may serve several useful functions for USAID as it considers what reforms to promote and where.

First, such benchmarks provide a starting point for discussion on whether a country should implement further reform in the particular area when other, more detailed, studies are unavailable. In fact, even when such studies are available, they tend to be country-specific and use different methodologies, making comparisons difficult. Our rules of thumb should therefore be helpful for mission directors either who are just arriving in-country or who want an independent, comparative assessment for a new sector.

Second, the rules of thumb should be a helpful *orienting* device for USAID/Washington staff who participate in parameter-setting meetings on diverse countries and sectors but do not have time for extensive preparation. Here the rules of thumb provide an independent–and cost-effective–reference point with which to evaluate the economic assessment or proposals in the country assistance strategy document. A country well below its threshold should exhibit positive

idiosyncrasies that compensate for apparent institutional inadequacies relative to other countries in its group.

Several caveats are in order, however. *First*, the operative term here is "orientating". While the rules of thumb describe the expected performance based on countries with a similar set of initial conditions, they are *not* an alternative to fielding a sector preparation team. As is the case with any set of indicators, they are a cost-effective expedient, not a substitute for country-specific case studies. Nevertheless, such benchmarks may be equally valuable to USAID/WDC during parameter-setting meetings. Here, the indicators would provide “red flags” for any mission whose strategy ignores such thresholds for its host country. The goal is not to reject the strategy of trade liberalization for a country but to call attention to cases where success would likely require the country in question to exhibit economic behavior very different from other countries with similar initial conditions. In such a case, the mission director would seek to explain why her country would be an exception to the rule of thumb. *Second*, for the trade liberalization application herein, we focus here on the *development* angle of trade capacity-building, not whether the country is WTO-compliant. Similarly, many promote trade liberalization for non-directly economic objectives, such as spread of democracy or regional integration to promote world peace. These latter are truly laudable goals, but are not explicitly considered in the toolkit.

5.2 Description of the toolkit

The toolkit centers around the rules of thumb in the form of the thresholds of institutional quality estimated above which country experience suggests are adequate for an existing degree of openness to lead to sustainable economic growth. Where institutional quality is “in excess” of what other countries with similar initial conditions had when their reforms produced economic gains, then further reform in the country under investigation *may* be beneficial; where it is not (or is just adequate) then further reform (e.g., trade liberalization) *may* not be successful and additional investment in the country’s institutions *may* be warranted first.

In each case we use the word “may” in Italics to emphasize that the tool is *not* a forecast, but a rule of thumb. The presumption is that where a USAID-sponsored reform is proposed and the rule of thumb is violated, then the proponent should be prepared to elucidate compensating, idiosyncratic factors arguing why her case should fly in the face of other countries’ experience.

In its most basic form, we would envision the toolkit to comprise a series of spreadsheets.[22] The Threshold Sheet would contain the most up-to-date values of the upper and lower thresholds (rules of thumb) for each country as well as the benchmark values of the policy regime (TPR in the case of our trade liberalization application) and law and order (LAO). The Simulation Sheet would contain formulas for each country so that if one enters two variables' values (from among the change in GDPPC, the policy regime indicator, and LAO) then the sheet computes the third. This is described further below. Both sheets could provide statistical and graphic output. Finally, both sheets could be utilized internationally by USAID missions either from the USAID intra- or Internet site or a CD provided directly to the missions by EGAT.

Based on these spreadsheets the toolkit would have an assessment and a scenario evaluation function. The assessment tool would provide inter-temporal and cross-country comparisons of institutional adequacy for further reform based on existing conditions. The inter-temporal comparisons would allow the USAID user to determine a country's reform history in the sector under question. For example, comparing values of the reform benchmark in two different periods would indicate how far a country had come in its reform efforts. Identifying the number of times the reform indicator hit a peak or trough would suggest the number of reform "reversals" experienced. Cross-country comparisons would permit USAID to consistently and directly compare the degree of policy reform in two or more countries. This would be helpful, say, when a mission staff member has extended experience in one country and arrives at another. We expect the preponderant use of this diagnostic, however, to be to evaluate the appropriateness of a country's existing conditions for further reform. This would be a simple matter of comparing the country's law and order benchmark to the threshold value "required" in order for reform to be growth-enhancing. These threshold values would be available for various time horizons.

Another type of assessment which can be optionally carried out is called a structural analysis. This entails examining the actual coefficients of the regressions in Table 9. These coefficients are invariant within an income cluster and across time, but do vary across clusters. Possible comparisons in this case would be to compare the relative size of the direct effect of an increase in the openness of the trade policy regime to the synergy effect modulated by the quality of law and order. Using the formula in equation (11) it is easy to understand the extent that the

[22] It is immaterial whether the spreadsheets are in separate files or simply sheets within one "workbook" (to use the MS-EXCEL term). Note that these sheets are in addition to the full panel dataset, containing all data used in carrying out the econometrics of Steps 3 and 4.

latter impact is important. Likewise, one could compare either of these two effects (coefficients) across income clusters. It is also very instructive to compare within a given cluster how the impact of trade liberalization changes as we consider its effect from the current year through to year 6.[23]

Table 1: Countries exhibiting the highest/lowest levels in their trade policy regime (TPR) for 1995-7 and the biggest changes in TPR for 1985-1997.

Benchmark	*Full Sample*	*Low Income*	*Lower Middle Income*	*Upper Middle Income*	*High Income*
Highest TPR Scores[b]	Singapore Hong Kong Trinidad	Gambia Malawi Zimbabwe	Jamaica Thailand Congo, Rep.	Trinidad Gabon Botswana	Singapore Hong Kong Belgium
Lowest TPR Scores[b]	Mali Mexico China	China Mali Niger	Algeria Columbia Iran	Mexico Argentina Turkey	Canada Japan Italy
Average Scores[b]	Panama Philippines Zambia	Bangladesh Togo Uganda	Philippines	South Africa	Denmark Netherlands Belgium
Biggest Improvement in Trade Policy[a]	Uganda El Salvador Indonesia	Uganda Sri Lanka Ghana	El Salvador Indonesia Morocco	South Africa Malaysia Turkey	South Korea Greece Singapore
Biggest Change Towards Protectionism[a]	Brazil Niger Cameroon	Cameroon Niger Madagascar	Papua New Guinea Algeria Ecuador	Brazil Botswana Uruguay	Hong Kong Belgium Canada
Average change[a]	Austria Denmark Mali	Zambia Ethiopia Zimbabwe	Peru Guatemala Paraguay	Saudi Arabia Costa Rica Chile	Netherlands Austria Denmark

[a]Difference between average 1995-97 and average 1985-87. [b]Average of 1995-97 averages by country.

While perhaps to be used less frequently, the scenario ("what if") evaluation tools would allow the end-user to set up a scenario for a country and simulate what the likely consequences would be under such conditions. For example, one could ask what the institutional threshold would have to be in order for an increase of one-tenth standard deviation in reform policy (as measured by the toolkit's reform indicator) to raise GDPPC by one percent. Such an exercise could be done for alternative time horizons. Likewise, a USAID mission might want to examine the impact of future complementary reforms on a country's preparedness for policy reform. In

[23] As discussed in section 4.1.2, we actually considered 9 and 12 years out in the trade liberalization application but, with few exceptions, few statistically significant effects were detected. Users of the methods here, however, would be encouraged to experiment with different time horizons.

this case, the toolkit user would enter the new (future) level of the complementary reform (LAO in the current example) and have the diagnostic tool determine the expected outcome.

5.3 Application of the toolkit to trade liberalization

Let us illustrate some of the use of the diagnostic toolkit in the context of USAID programming for the case of trade liberalization, as analyzed in section 4. In so doing we can draw out the policy implications from the trade liberalization application. The toolkit comprises two sets of diagnostics, assessment and scenario evaluation.

5.3.1 Assessment

We may begin the assessment with simple examples of how the toolkit could provide inter-temporal and cross-country comparisons of a country's trade openness stance. Table 1 uses the series of estimates of the trade policy regime (TPR) to indicate the countries *within* each income group with particular *extrema* levels or changes-in-levels for the group. The top three rows indicate countries with the highest, average, and lowest levels of trade openness for their respective group, which a USAID mission may use to compare their own country. The bottom three rows of the table illustrate inter-temporal comparisons by comparing values of the trade policy regime over the period 1985 to 1997 to indicate tangibly how far a country has come (or slipped back) in its reform efforts. Of course, the mission might use the same data to rank its country against any other subset of countries in the region or criterion of their choice, taking account existing conditions.

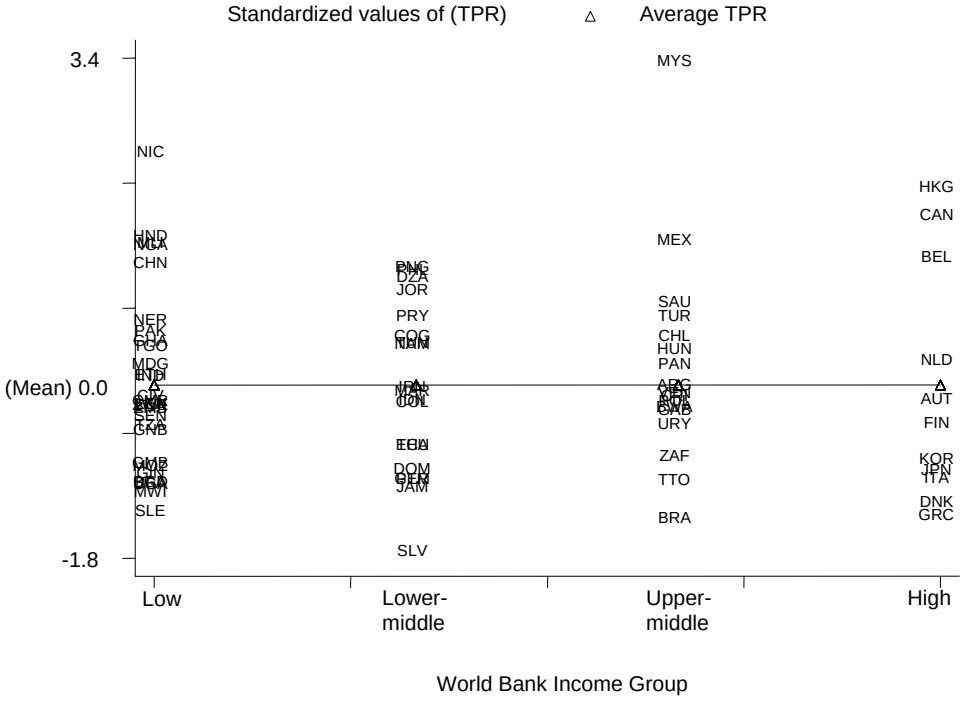
Another easy way to make cross-country comparisons is shown in Figure 3 and Figure 4. These graph our trade policy measure, TPR, for each of groups of countries based on income level. In Figure 3 for 1996 we see that, Malaysia aside, the two middle-income groups have the least dispersion of TPR, the best and worst performers are Malaysia and El Salvador. It is also reveals that each cluster had the same average value and that this value was in fact the average for the whole sample across all years, namely, zero. In Figure 4, we see that, as expected, the high-income group has the highest level quality of its trade regime on average. We also observe that the trend by income group is quite smooth over time, though the bottom three income groups display an upward trend, commencing in 1991.[24] What is of particular interest is that the upper-middle income group displays the least improvement over the period, leading to the surprising

[24] This is likely related to the demonstration effects and geography-politics of the "fall" of the Soviet Union.

result that the lower-middle income group, which began the period as the second worst performer on average, improves so much that it overtakes the upper-middle income group in the quality of its trade regime.

Of course the principal interest in this diagnostic toolkit is the thresholds of requisite institutional quality for growth-generating trade liberalization. USAID/WDC will want to use these for their country strategy plans to determine whether the country is ready for further trade liberalization or whether additional trade capacity building is required.[25] Similarly, USAID can refer to them as rules of thumb and a point of departure in parameter-setting meeting discussions about a mission's trade liberalization strategy for a country.

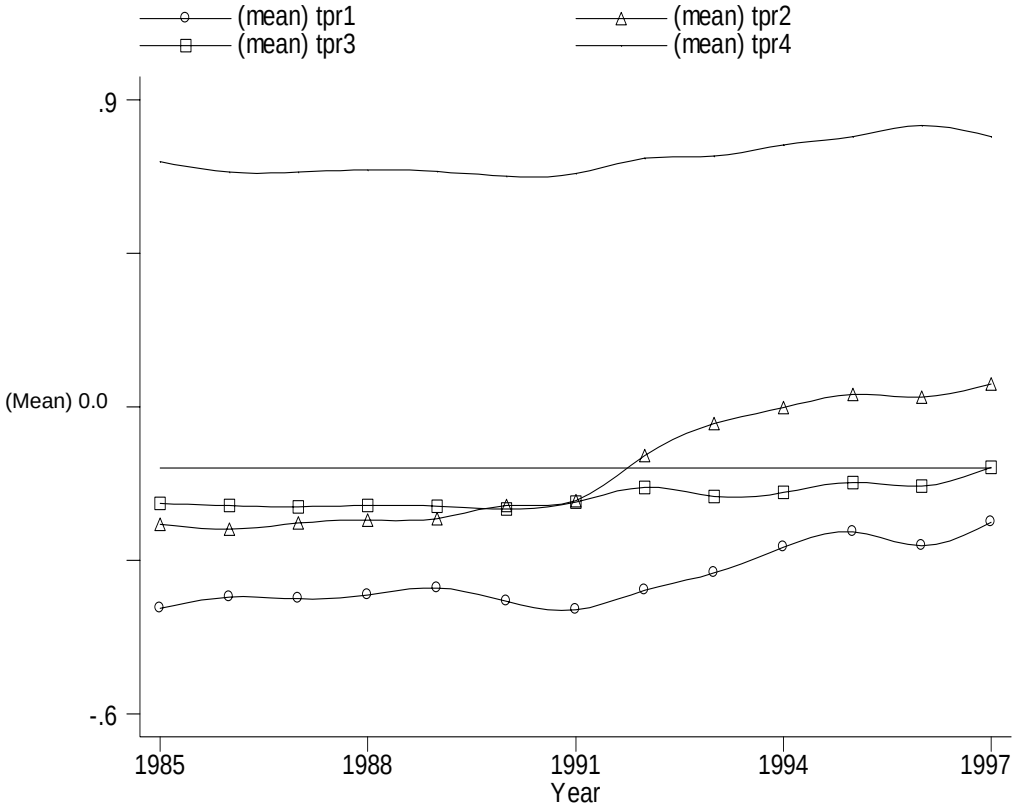
Figure 3: Dispersion of trade policy regime by income group for 1996.



Source: Authors' calculations from the State-I regressions.

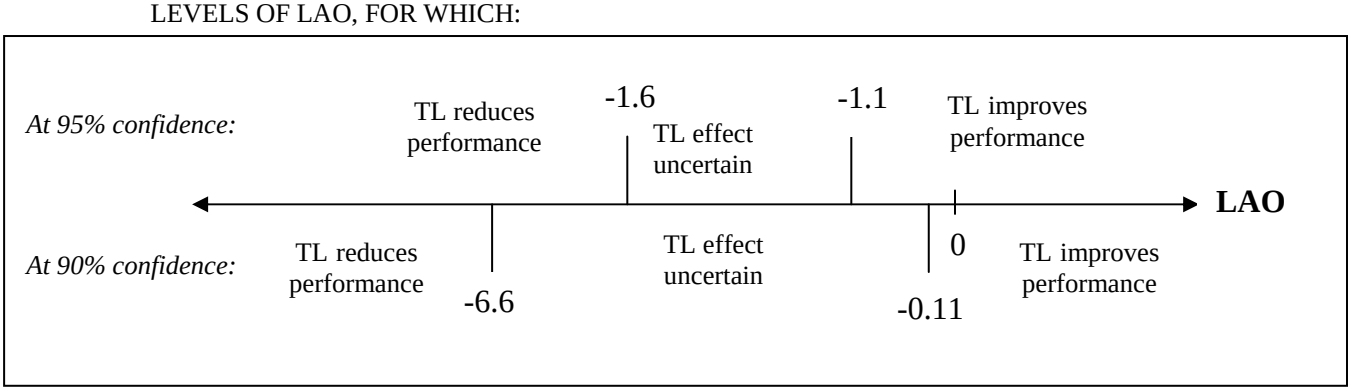
[25] We are not proposing nor would we encourage USAID to rely exclusively on these indicators. They are simply a useful counterfactual prior to contracting a team of trade policy experts.

Figure 4: The evolution of trade policy regimes by income group, 1985-1997.



Notes: Horizontal line indicates the mean value for TPR over all countries and years. Tpr1 to tpr4 are the average values of TPR for the low through high income groups, respectively.
Source: Authors' calculations from the State-I regressions.

Figure 5: Values of Law & Order (LAO) required for an increase in trade liberalization (TL) to generate contemporaneous gains or losses in GDP (from full sample).



Source: Authors' estimates, using one-sided F-test for coefficients estimated in regression (10) in Table 9.

The results of these tests for confidence levels of 90 and 95 percent are shown in Figure 5 for the requirements of a statistically significant contemporaneous impact. As an example, at the 10-percent significance level, for any country with a level of LAO above -1.1, (i.e., a bit below one standard deviation below the sample mean across all countries and years), *any* TPR liberalization will cause a contemporaneous *gain* in economic performance. Similarly, for any country with a level of LAO below -1.6 (i.e., a bit less than one and a half standard deviations below the sample mean across all countries and years), *any* TPR liberalization will cause a *loss* in economic performance at the 5-percent significance level. We have presented threshold estimates at both levels of confidence—LAO within the intervals (-6.6,-0.11) at the 90-percent level and within the interval (-1.6,-1.1) at the 95-percent level—to illustrate how the bands of statistical uncertainty become more severe (i.e., more countries enter the band) as smaller significance levels are selected. For the rest of this paper we stick to thresholds based on the 95-percent confidence level.

Table 2 presents these critical thresholds of LAO, our complementary institutional quality proxy for trade liberalization, for the full sample of 80 countries and by World Bank income cluster.[26] The reader may wish to review Figure 1 for a summary of the values of LAO in the sample. As in Figure 5, it indicates the highest and lowest values of the LAO environment required for us to be able to say unambiguously (i.e., at the 95-percent level of confidence) whether the effects on GDPPC of trade liberalization are unambiguously negative or positive. The table confirms that the thresholds in Figure 5 based on the full sample obscure substantive variation across income groups.

[26] The bands in this table depend on the statistical significance level chosen. We have selected a level so that there would only be a five-percent chance (95-percent confidence) that we observe a country in the uncertainty region and benefit from trade liberalization. The ten-percent level of significance (90-percent confidence), being less stringent, allows for a smaller uncertainty band and therefore admits more countries into the positive and negative impacts zones. We have included it here as an illustration. Unless noted otherwise, below we continue using a five-percent level of significance.

Table 2: Thresholds of institutional quality (LAO) for a positive (negative) impact of trade liberalization on economic performance, by income level and delay

Year of impact	*Full Sample*		*Low Income*		*Lower-Middle Income*		*Upper-Middle Income*		*High Income*	
	<Loss	*>Gain*	*<Loss*	*>Gain*	*<Loss*	*>Gain*	*<Loss*	*>Gain*	*<Loss*	*>Gain*
Current period	-1.6	-1.1	Dius	0.3	-1.58	0.26	-0.2	Dius	0.5	2.25[†]
Year 3	Dius	1.35	Dius	Dius	-1.59	-0.37	Dius	Dius	0.06	1.79
Year 6	Dius	Dius	-0.74	0.3	Dius	0.51	-1.75	1.23	-0.68	1.18

Source: Authors' calculations, using one-sided F-test at the 95-percent confidence level for coefficients estimated for equation (10) and shown in Table 9.

Notes: Thresholds are measured in standard deviations relative to the mean level (set at zero) of institutional quality for the full sample over the period 1985-1997. LAO values falling between the Loss and Gain thresholds are a region of statistical uncertainty. "Dius" means that the *d*irection of *i*mpact is *u*ncertain statistically, i.e., the data are not sufficient to reduce the uncertainty for zone of impact to a level which could be relevant for a country in the respective (sub) sample. See Table 6 for income group definitions. [†]This value is outside the range of all the countries in our sample.

Dropping down to the level of the income cluster, several notable features appear. For the lowest income group, only countries which undertake trade liberalization with the very worst-quality institutions in the same period—over one and a half standard deviations below the mean for all countries and years—are unambiguously likely to experience contemporaneous economic decline from trade liberalization. By Year 3, when the reallocative efficiency-enhancing effects of trade liberalization have had time to work themselves through the economic system, the lower threshold of LAO for a negative outcome has fallen so low that it is no longer measurable. In other words, *no* countries in this group were unambiguously likely after three years to be worse off at the national level from trade liberalization. On the other hand, any low-income country with a level of LAO of slightly more than one-and-a-third standard deviations above the full-sample mean will unambiguously experience a contemporaneous increase in GDPPC from trade liberalization. Finally, low-income countries with a level of LAO three-quarters of a standard deviation below the full-sample mean at the onset of trade liberalization should unambiguously expect to experience by Year 6 a *decrease* in GDPPC. Likewise, low-income countries starting with a level of LAO of almost one-third of a standard deviation above the full-sample mean at the onset of trade liberalization should expect to experience by Year 6 an unambiguous *increase* in GDPPC. For 1991 the only country we are confident would have gained from trade liberalization would have been Tanzania—and only in the year of the liberalization. Many countries, however, would have unambiguously lost from a 1991 trade liberalization by 1997. By 1997, however, the quality of law and order in the low-income countries was such that no countries would

lose in the long run—though none would gain either. In the short run, though, seventeen out of the 31 countries in our sample for this group would have experienced a contemporaneous gain from a trade liberalization in 1997.

For the lower-middle income group, countries which undertake trade liberalization with the an institutional quality in the same period worse than over one and a half standard deviations below the mean for all countries and years are unambiguously likely to experience contemporaneous economic decline from trade liberalization. Any of these countries with an level of LAO a bit above a quarter of a standard deviation above the full-sample mean should experience an unambiguous increase in contemporaneous economic activity from trade liberalization By Year 3, while this lower threshold of LAO for a negative outcome has not changed, any lower-middle-income country with an average level of LAO over the three years of slightly below one-third standard deviations *below* the full-sample mean will unambiguously experience a contemporaneous increase in GDPPC from trade liberalization. Thus, a level of LAO below -0.37, while not enough for an immediate GDPPC improvement, can eventually facilitate trade liberalization. Finally, lower-middle-income countries with an average level of LAO half-a-standard-deviation above the full-sample mean over the six years after the trade liberalization should unambiguously expect to experience an increase in GDPPC. For 1991 the only country we are confident would have lost from trade liberalization would have been South Africa. Otherwise many countries—as Table 3 lists—would have benefited in the current, short and long-run from trade liberalization. The situation was even rosier in 1997, where our results suggest that there would not likely have been any country in the group which would have lost from trade liberalization. Rather, Table 4 indicates that most—18 out of our sample of 19—countries in the group would have gained from it.

For the upper-middle-income countries, no systematic evidence for a positive income effect in the year of the reform was detected. This suggests that these countries would probably not do well contemporaneously from trade liberalization. Those with an institutional quality below a fifth of a standard deviation below the full-sample mean, however, would be unambiguously worse off in the period of the trade liberalization. For Year 3 our data were not good enough to separate the winners and loser from trade liberalization. By Year 6, however, only countries below one and three-quarters of a standard deviation below the full-sample mean would be unambiguously worse off by Year 6. On the other hand, any country above one and a

quarter standard deviations of the full-sample mean would be unambiguously better off by Year 6. From Table 3, we see for 1991 that 9 out of 18 would have lost from trade liberalization in the same year. Turning to Table 4 we see for a trade liberalization in 1997 that in fact no countries had levels of LAO so low as to have been loser from trade liberalization in any subsequent period. As for gainers, we only found that three countries (Hungary, Argentina and Chile) would have gained—and then only by Year 6 (2003).

For the high-income countries, the level of LAO required to lose from trade liberalization fell as time elapsed, starting from half a standard deviation in the contemporaneous period of trade liberalization to a bit above the mean (zero) by three years after the reform to a bit below two-thirds of a standard deviation below the mean by Year six. A similar pattern occurs for the bar required to gain from trade liberalization: an contemporaneous improvement from trade liberalization requires a rather high "bar" for institutional quality at two and a quarter standard deviations above the full-sample mean, falling to one and four-fifths standard deviation by Year 3 and one and a fifth standard deviations by Year 6—almost half that which was required in the period of the trade liberalization. Given that the lower threshold for losses was below the institutional quality of all these countries, these results suggest that the top-tier countries probably don't lose from trade liberalization. However, the relatively high but falling upper threshold for gains from trade liberalization suggest that these countries have complex economies which take considerable time to adjust to trade liberalization. In fact by Year 6 after liberalization, most high-income countries will benefit in the long-run from trade liberalization. From Table 3, we see for 1991 that only 3 out of 13 countries would have likely lost from trade liberalization in that same year or by 1994; none would have lost by the year 1997, however. On the other hand, while none would have gained in the short run, by the year 1997 a trade liberalization in 1991 would have generated gains in over half the high-income countries of our sample. Turning to Table 4 we see for a trade liberalization in 1997 that no countries in our sample would have lost from it and, again while none would have gained in the short run, 12 out of 13 countries would have gained from it.

In conclusion, whether pro- and anti-globalization forces will see these results as vindications of their position one can only speculate.

Table 3: Country losers and gainers from the effects of trade liberalization in 1991 by income and delay

Income Group	*TL Effect*	*In 1991 (Current period)*	*By 1994 (Year 3)*	*By 1997 (Year 6)*
Full Sample	Losers	Sri Lanka, Ethiopia, Bangladesh, Colombia, El Salvador, Guatemala, Guinea-Bissau, Haiti, Pakistan, Peru, Philippines, South Africa, Uganda, Zambia	Dius	Dius
	Gainers	Kuwait, Nigeria, Congo, Rep., Honduras, Jamaica, Malawi, Mali, Morocco, Namibia, Nicaragua, Niger, Panama, Paraguay, Senegal, Togo, Tunisia, Zimbabwe, Turkey, Iran, Sierra Leone, Burkina Faso, Indonesia, Cameroon, Gabon, Jordan, Madagascar, Korea, Rep., Argentina, China, Cote d'Ivoire, Dominican Republic, Gambia, Ghana, Guinea, Kenya, Malaysia, Mexico, Papua New Guinea, Saudi Arabia, Uruguay, Brazil, Chile, Costa Rica, Ecuador, Greece, Hong Kong, Oman, Poland, Tanzania, Thailand, Trinidad and Tobago, Venezuela, Botswana, Hungary, Italy, Japan, Singapore, Austria, Belgium, Canada, Denmark, Finland, Netherlands, New Zealand	Austria, Belgium, Canada, Denmark, Finland, Netherlands, New Zealand	Dius

Notes: See end of table.

Finally, let us examine Table 9 to illustrate the use of structural analysis. This comparison of coefficients leads us to support several sets of conclusions.

First, we see that, with the exception of the low-income cluster in Year 3 and the upper-middle-income cluster in the contemporaneous period of liberalization, the TPR variable *on its own* in Table 9 is generally insignificant (or, actually *negative* in impact for the high-income countries in all periods). This means that trade liberalization *alone* is not enough to generate short-term gains at the economy-wide level.

Table 3: Country losers and gainers from the effects of trade liberalization in 1991 by income and delay (continued)

Income Group	*TL Effect*	*In 1991 (Current period)*	*By 1994 (Year 3)*	*By 1997 (Year 6)*
Low Income	Losers	Dius	Dius	Sri Lanka, Ethiopia, Bangladesh, Guinea-Bissau, Haiti, Pakistan, Uganda, Zambia, Mozambique, India, Kuwait, Nigeria, Mali, Malawi, Nicaragua, Niger, Senegal, Togo, Zimbabwe
	Gainers	Tanzania	Dius	Dius
Lower Middle Income	Losers	South Africa	South Africa	Dius
	Gainers	Brazil, Chile, Costa Rica, Oman, Poland, Trinidad, Venezuela, Botswana, Hungary	Argentina, Malaysia, Mexico, Saudi Arabia, Uruguay, Brazil, Chile, Costa Rica, Oman, Poland, Trinidad, Venezuela, Botswana, Hungary	Botswana, Hungary
Upper Middle Income	Losers	South Africa, Panama, Turkey, Gabon, Argentina, Malaysia, Mexico, Saudi Arabia, Uruguay	Dius	None
	Gainers	Dius	Dius	None
High Income	Losers	South Korea, Greece, Hong Kong	South Korea, Greece, Hong Kong	None
	Gainers	None	None	Austria, Belgium, Canada, Denmark, Finland, Netherlands, New Zealand

Source: Authors' estimates, comparing country values to thresholds at the 95-percent level from Table 2.
Notes: "Dius" – Direction of impact uncertain statistically. See Table 6 for income group definitions.

Second, it is probably not wise for USAID to apply a "one-size-fits-all" policy to the countries it advises. As we can see, countries in different income groups react rather differently to the same policy reform, in this case trade liberalization. For example, the direct effect (ignoring institutional quality) of trade liberalization has no contemporaneous statistical impact on GDPPC for the low- income cluster, a positive one for upper-middle-income countries, and a statistically significantly negative one for high-income countries.

Table 4: Country losers and gainers from the effects of trade liberalization in 1997 by income and delay

Income Group	*TL Effect*	*In 1997 (Current period)*	*By 2000 (Year 3)*	*By 2003 (Year 6)*
Full Sample	Losers	None	Dius	Dius
	Gainers	Whole Sample	Namibia, Hungary, New Zealand, Finland, Singapore, Denmark, Austria, Netherlands, Canada, Italy, Japan, Greece, Hong Kong,	Dius
Low Income	Losers	Dius	Dius	None
	Gainers	China, Tanzania, Cote d'Ivoire, Ethiopia, Gambia, Kuwait, Malawi, Zambia, Uganda, Burkina Faso, Zimbabwe, Haiti, Mozambique, Ghana, Sri Lanka, Nicaragua, Kenya	Dius	None
Lower-Middle Income	Losers	None	None	Dius
	Gainers	Namibia, Morocco, Tunisia, Thailand, Paraguay, Dominican Republic, Ecuador, Guatemala, Peru, Jamaica, El Salvador, Indonesia, Iran, Papua New Guinea	Namibia, Morocco, Tunisia, Thailand, Paraguay, Dominican Republic, Ecuador, Guatemala, Peru, Jamaica, El Salvador, Indonesia, Iran, Papua New Guinea, Jordan, Philippines, Colombia, Congo, Rep.	Namibia, Morocco, Tunisia, Thailand, Paraguay, Dominican Republic, Ecuador
Upper-Middle Income	Losers	None	Dius	None
	Gainers	Dius	Dius	Hungary, Argentina, Chile
High Income	Losers	None	None	None
	Gainers	None	None	New Zealand, Finland, Singapore, Denmark, Austria, Netherlands, Canada, Italy, Japan, Greece, Hong Kong, Belgium

Source: Authors' estimates, comparing country values to thresholds at the 95-percent level from Table 2.
Notes: "Dius" – Direction of impact uncertain statistically. See Table 6 for income group definitions.

Third, the net effect of trade liberalization depends on the quality of the country's enabling environment. Moreover, its synergizing effect is either positive or negative, depending on whether the country is in the bottom two or top two clusters. If accompanied by institutional strengthening of the enabling environment, trade liberalization seems to have a more negative effect for the bottom two clusters and a more positive effect on the top two clusters. In other words, for countries with weak complementary institutions—those roughly below one-and-a-half

standard deviations below the mean for the full sample, trade liberalization can actually lead to *loss* of GDPPC.[27] (Note, however, that it has been outside our scope to identify the exact nature of what that complementary institutional strengthening might entail.)

Fourth, each cluster seems to have its own trend for the peak *indirect* trade liberalization effect over time. For the low-income cluster, the only statistically significant indirect effect from trade liberalization is in the short run. For the lower-middle-income cluster trade liberalization seems to have a stable and statistically significant negative effect over time. For the upper-middle-income cluster, the indirect effect is statistically significantly positive in the year of the reform and in Year 6, though twice as strong in the latter. Finally, for the high-income cluster, the indirect effects in all periods are statistically significantly positive, though strongest in Year 6.

5.3.2 Scenario evaluation

We anticipate that the toolkit could be used to answer "what if" questions to allow USAID to set up a scenario for a country and simulate what the likely consequences would be under such conditions. For example, one could ask what the institutional threshold would have to be in order for an increase of one-tenth standard deviation in reform policy (as measured by the toolkit's reform indicator) to raise GDPPC by one percent. Such an exercise could be done for alternative time horizons. Likewise, a USAID mission might want to examine the impact of future complementary reforms on a country's preparedness for policy reform. In this case, the toolkit user would enter the new (future) level of the complementary reform (LAO in the current example) and have the diagnostic tool determine the expected outcome.

5.4 Toolkit maintenance

We propose that USAID (ideally) engage in several types of maintenance annual in order to ensure that the diagnostic toolkit remains up to date and maximally effective. We believe that in all likelihood this activity can be carried out by the CDIE, possibly with the help of the DEC.

First, USAID should ensure that the database upon which the toolkit relies is kept current. (The data requirements for the trade liberalization application are summarized in section 4.1 and described in more detail in Table 5.) Due to its small number of variables involved, this activity would not take more than a half day per year.

[27] This is theoretically possible even for high-income quality institutions.

Second, the econometrics for estimating the stage-I and stage-II regressions as well as for the threshold (rules-of-thumb) computations should be re-estimated annually once the database has been updated.[28] The software could be provided for this purpose which would then automatically re-compute the institutional thresholds. While this re-estimation would not require advanced expertise, we do recommend that once every two or three years the process be done by an outside expert (at one day of consulting time per update) to ensure that the econometric specification remain maximally relevant.

Finally, the new country threshold tables (spreadsheets) should be placed on the USAID Intranet for ease of use in the field.

6 Conclusions

How much reform can a country take and still have a positive impact on economic growth? As veteran reformers always suspected and unlike the dictum of Economics 101, "more is *not* necessarily better". For as many as there are reform success stories, we find an equal number in which the impact of reform has been at best neutral and in many cases discernibly negative. A concrete example is privatization in the former centrally planned economies. According to a recent USAID study (Zinnes *et al.* 1999), the macroeconomic impact of privatization was not uniformly positive. It turned out that over the decade of the 1990s, only where the underlying supporting institutions were of adequate quality was privatization growth-enhancing.

In the present paper we build on this insight and propose a method for USAID to gauge the likely impact at the macroeconomic level of further reform, i.e., whether more reform would likely be growth-enhancing. The method is based on an evaluation of the combined past reform experience of countries for the sector under consideration. We do this by creating an indicator of policy, computed as the (standardized variance of the) unexplained performance of the sector under analysis once non-discretionary characteristics (e.g., culture, geography, climate, quality of rule of law) are purged. We then use the indicator to benchmark countries over time, both compared to all countries as well as to the relevant country cluster. Then, by analyzing country scores relative to macroeconomic performance, we are able to compute country-specific thresholds, based on country characteristics that indicate when further reform is likely to improve, worsen, or have uncertain effect on macroeconomic performance.

[28] IRIS has written a computer "do" program in STATA 7 for this purpose which, upon request could be provided.

These country benchmarks and thresholds, which one can think of as "rules of thumb", may serve several useful functions for USAID. First, they provide a starting point for discussion on whether a country should implement further reform in the particular area when other, more detailed, studies are unavailable. (In fact, even when such studies are available, they tend to be country-specific and use different methodologies, making comparisons difficult.) Our rules of thumb should therefore be helpful for mission directors either who are just arriving in-country or who want an independent, comparative assessment for a new sector. Second, the rules of thumb should be a helpful *orienting* device for USAID/Washington staff who participate in parameter-setting meetings on diverse countries and sectors and don't have time for extensive preparation. Here the rules of thumb provide an independent–and cost-effective–reference point with which to evaluate the economic assessment or proposals in the country assistance strategy document. A country well below its threshold should exhibit positive idiosyncrasies that compensate for apparent inadequacies relative to other countries in its group. However, the operative term here is "orientating". While the rules of thumb describe the expected performance based on countries with a similar set of initial conditions, they are *not* an alternative to fielding a sector preparation team.

As an example of how USAID might use the techniques above, we describe and then illustrate a diagnostic toolkit which applies our methodology to the current debate on whether it is wise for a particular country to undergo further trade liberalization. As is often the case in such controversies, we find that both sides are "right"—depending on the initial conditions. Starting with our full sample of 80 countries, we first show at the 95-percent confidence level that trade liberalization *alone* is not likely to be enough to generate macroeconomic performance improvements over the ensuing six years thereafter. In fact if anything it seems to have a *negative* albeit often statistically insignificant impact, on average, for at least the first three years after the reform. Dropping down to the level of The World Bank income (WBI) groups, we find that this result is largely borne out—for example the effect is quite negative for high-income countries regardless of the length of time after the reform. The one exception is the upper-middle income group, which would experience a contemporaneous improvement in the period of the reform but not thereafter. In short, trade liberalization *by itself* seems to have no effect on the low- and lower-middle-income groups and a negative effect on the highest income group. We then present analysis to suggest a more nuanced view, namely, that if a country's institutions of law and order

exceed a certain level of adequacy (that is, the thresholds mentioned above and which we calculate by income group) then trade liberalization can indeed lead to gains in economic performance. While a trade liberalization in 1991 would have likely led 19 out of 31 low-income countries to experience losses in economic performance by 1997, a trade liberalization in 1997 would have brought, because of the improvements in law and order over the intervening 6 years, macroeconomic gains to 18 out of 31 countries and with *none* of the group likely to experience a loss.

Nonetheless, even allowing for institution quality, we still find that trade liberalization can have a negative economic impact for some countries. For example, based on thresholds computed for the full sample, any country whose quality of law and order was a bit over one-and-a-half standard deviations below the mean, would have likely experienced a negative economic impact in the year of the liberation. The paper computes more precise thresholds (both for likely gains as well as losses) by income group and indicates which countries would fall into each case.

In sum, one size policy does *not* fit all and its impact depends on initial conditions—and on the quality of supporting institutions, in particular. The toolkit we present herein provides a practical and low-cost way for USAID to start the process of targeting its policy assistance more effectively.

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Appendices

Table 5: List of variables and sources of data used in the analysis

Variable name	*Description*	*Source*
AREA	Land area (in square kilometers) is a country's total area, excluding area under inland water bodies, national claims to continental shelf, and exclusive economic zones.	WDI
DISTANCE	To one of five major international ports	Online database, Center for International Development (CID), Harvard University
OIL	Dummy variable equal to 1 if country is an oil exporter; zero otherwise	Online database, Center for International Development (CID), Harvard University
WB	Cluster dummies representing The World Bank four income groups (see Table 6).	WDI
COAST	Percent of country within 100 kilometers of a coast line	WDI
ENGLISH, SPANISH, FRENCH	Dummy variables equal to 1 if English, Spanish, or French is an official language of the country; zero otherwise	WDI
GDPPC	Gross domestic product per capita measured in constant 1999 dollars	WDI
AG, MANU	Share of agricultural and manufacturing in GDP	WDI
ISLAND	Dummy variable equal to 1 if country is an island; zero otherwise	WDI
LAO	ICRG variable on the degree of rule of law (law and order); takes on the values 1 to 6, with 6 being the "best"	ICRG
LLOCK	Dummy variable equal to 1 if country is land-locked; zero otherwise	WDI
TPR	Trade policy regime	Estimated by the authors as the residual from from regression (9)
TRDSHR	The sum of exports and imports of goods and services measured as a share of gross domestic product.	Constructed from WDI

Table 6: The World Bank income groups The World Bank income groups

Cluster name	*GDPPC**	*No. of countries*	*Countries in group*
Lower	< $755	31	Sri Lanka, Ethiopia, Bangladesh, Guinea-Bissau, Haiti, Pakistan, Uganda, Zambia, Mozambique, India, Kuwait, Nigeria, Honduras, Malawi, Mali, Nicaragua, Niger, Senegal, Togo, Zimbabwe, Sierra Leone, Burkina Faso, Cameroon, Madagascar, China, Cote d'Ivoire, Gambia, Ghana, Guinea, Kenya, Tanzania
Lower-middle	$756-$2995	19	Colombia, El Salvador, Guatemala, Peru, Philippines, Algeria, Congo (Rep.), Jamaica, Morocco, Namibia, Paraguay, Tunisia, Iran, Indonesia, Jordan, Dominican Republic, Papua New Guinea, Ecuador, Thailand
Upper-middle	$2996-$9265	18	South Africa, Panama, Turkey, Gabon, Argentina, Malaysia, Mexico, Saudi Arabia, Uruguay, Brazil, Chile, Costa Rica, Oman, Poland, Trinidad, Venezuela, Botswana, Hungary
Upper	> $9266	13	South Korea, Greece, Hong Kong, Italy, Japan, Singapore, Austria, Belgium, Canada, Denmark, Finland, Netherlands, New Zealand

*GDPPC = Per capita income range of group

Table 7: Correlations between trade policy regime (TPR) and widely used measures of a country's openness, 1997.

	TPR	Dollar Index	Black Market Premium	Average Tariffs	Non-Tariff Barriers
TPR	1				
Dollar Index	0.1115 (0.3729) [66]	1			
Black Market Premium	-0.0122 (0.9284) [57]	0.2039 (0.1318) [56]	1		
Average Tariffs	0.1005 (0.5266) [42]	-0.1688 (0.3042) [39]	0.4464 (0.0081) [34]	1	
Non-Tariff Barriers	0.4728 (0.1676) [10]	0.4079 (0.2759) [9]	0.3793 (0.2797) [10]	-0.0874 (0.8521) [7]	1

Notes: Number in parentheses is the probability or P-value, computed at the 5-percent level of significance. The number in square brackets is the number of observations.

Sources: TPR-Authors; Dollar index and the black market premium-The World Bank's Global Development Network Growth Database; Average tariffs-WBI; Non-tariff barriers-WBI.

Table 8: Gravity model of trade share whose residual predicts trade policy regime (Stage 1 regressions).

Dependent Variable: TRD_SHR	*Full Sample*		Low-income countries	Lower-middle income	Upper-middle income	High income countries
	1	2				
LN AREA	-0.5789	-0.5874	-0.2672	-0.258	0.0664	-1.236
	*(23.21)***	*(22.77)***	*(9.14)***	*(8.20)***	*(1.36)*	*(14.20)**
LAW AND ORDER	0.198	0.1903	0.2051	0.1093	0.0990	0.0370
	*(6.97)***	*(5.93)***	*(6.91)***	*(3.88)***	*(1.40)*	*(0.41)*
LANDLOCK	-0.056	-0.0562	-0.0229	0.2053	0.2894	-0.0445
	*(2.47)***	*(2.55)***	*(1.35)*	*(5.33)***	*(5.07)***	*(1.08)*
TROPICAL	0.3161	0.322	0.1561	0.0517	0.1557	0.4151
	*(10.51)***	*(10.55)***	*(3.61)***	*(1.20)*	*(3.88)***	*(3.01)***
ISLAND	-0.1549	-0.1835	0.0385	-0.0977	.4481	-0.1146
	*(6.78)***	*(7.94)***	*(1.55)*	*(3.22)***	*(8.35)***	*(2.35)**
DISTANCE	0.1463	0.2045	-0.0449	0.1042	-0.0053	0.5269
	*(5.79)***	*(7.52)***	*(1.18)*	*(2.59)***	*(0.14)**	*(8.19)***
FRENCH	-0.001	0.0239	-0.0559	0.1686	0.1676	1.0384
	(0.04)	*(0.91)*	*(2.28)**	*(3.31)***	*(2.64)***	*(10.37)***
ENGLISH	0.016	0.009	-0.0085	0.1721	.2740	-0.3790
	(0.62)	*(0.38)*	*(0.36)*	*(6.02)***	*(3.83)***	*(5.89)**
SPANISH	-0.265	-0.287	0.0348	-0.2901	0.0987	
	(10.16)***	(10.66)***	(0.89)	(8.54)***	(2.55)***	
AG	-0.335	-0.244	-0.2614	-0.4099	.8384	-1.2010
	*(10.54)***	*(6.40)***	*(9.14)***	*(8.96)***	*(5.58)***	*(3.23)**
OIL	0.0159	0.015	0.1328	-0.2052	0.0335	
	(0.62)	*(0.56)*	*(3.61)***	*(8.66)***	*(0.81)*	
MANUF	0.0861	0.0849	-0.0717	-0.2653	-0.2355	0.1516
	*(3.06)***	*(3.07)***	*(2.70)***	*(8.45)***	*(5.41)***	*(2.31)**
Observations	997	997	377	242	213	165
Cluster Dummies	No	Yes	No	No	No	No
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.59	0.61	0.55	0.85	0.61	0.96

Notes: * significant at 5%; ** significant at 1% absolute value of t statistics in parentheses; dependent variable is TRD_SHR, the ratio of imports plus exports over GDP.

Table 9: The effects of trade policy regime (TPR), institutions (LAO), and their interaction (LAOTPR) on GDP per capita (Stage-2 regressions) in the current period and 3 and 6 years later.

Explanatory Variable	*Low-Income Countries*			*Lower-Middle-Income Countries*			*Upper-Middle-Income Countries*		
	Current	Year 3	Year 6	Current	Year 3	Year 6	Current	Year 3	Year 6
TPR	-0.01488	0.0173	-0.0071	-0.007	-0.0032	-0.0092	0.0247	-0.0183	0.0054
	(1.64)	*(1.63) **	*(0.46)*	*(1.46)*	*(0.57)*	*(1.41)*	*(2.29)***	*(1.44)*	*(0.40)*
LAO, ALAO†	0.01157	0.0202	.0223	0.0033	0.0056	0.0048	0.004	-0.0067	0.0082
	*(2.41)***	*(2.69)****	*(2.13)***	*(1.38)*	*(1.41)*	*(0.86)*	*(0.63)*	*-0.60)*	*(0.67)*
ALAO_TPR†	-0.014	0.0062	-.0047	-0.0114	-0.0111	-0.0141	0.0116	0.0001	.0299
	*(1.93) ***	*(0.81)*	*(0.40)*	*(4.14)****	*(3.80)****	*(3.69)****	*(1.70)**	*(0.01)*	*(3.06)****
Observations	375	284	202	242	184	116	213	161	115
Countries	31	31	31	19	19	19	18	18	18
R-squared	0.94	0.95	0.95	0.85	0.86	0.98	0.94	0.92	0.94

Explanatory Variable	*High-Income Countries*			*Full Sample*		
	Current	Year 3	Year 6	Current	Year 3	Year 6
TPR	-0.1156	-0.1585	-0.1786	-0.0168	-0.0262	0.0014
	*(2.74)****	*(4.02)****	*(3.20)****	(1.57)	*(2.09)***	*(0.09)*
LAO or ALAO†	0.0291	0.0921	0.1527	-0.0137	-0.0164	-0.0061
	(1.09)	*(2.17)***	*(3.10)****	*(2.32)***	*(-1.65)**	*(0.50)*
ALAO_TPR†	0.0906	0.0744	0.1679	0.0093	-0.0081	0.0242
	*(3.54)****	*(2.67)****	*(5.74)****	*(1.36)*	*(0.99)*	*(2.45)***
Observations	165	126	88	995	755	534
Countries	13	13	13	81	81	81
R-squared	0.98	0.98	0.98	0.99	0.99	0.99

Source: Authors' calculations from equation 10.
Notes: All regressions contain fixed effects for The World Bank income groups, country and time fixed effects. For variable names, see Table 5. The t-statistics are in parentheses: † For the "Current" period regressions, this refers to LAO while for the lagged regressions it refers to ALAO. *, **, and *** indicate confidence levels of 90, 95, and 99 percents. Where cluster dummies have been used for the stage II regression, we have also used them in stage I.